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THE
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EDITED BY

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TABLE OF CONTENTS.

B. R. ANDREWS,	
Habit,	121-149
J. W. BAIRD,	
The Influence of Accommodation and Convergence upon the Perception of Depth,	150-200
H. BEAUNIS,	
Contribution à la psychology du rêve,	271-287
I. MADISON BENTLEY,	
The Simplicity of Color Tones,	92-95
Professor Calkins on Mental Arrangement,	113-114
A Critique of 'Fusion,'	324-336
JOHN A. BERGSTRÖM,	
A New Type of Ergograph, with a Discussion of Ergo- graphic Experimentation,	510-540
T. L. BOLTON,	
The Relation of Motor Power to Intelligence,	615-631
EDWARD FRANKLIN BUCHNER,	
A Quarter Century of Psychology in America, 1878-1903,	666-680
WM. H. BURNHAM,	
Retroactive Amnesia: Illustrative Cases and a Tentative Explanation,	382-396
J. MCKEEN CATTELL,	
Statistics of American Psychologists,	574-592
ALEXANDER FRANCIS CHAMBERLAIN,	
Primitive Taste-Words,	410-417
F. B. DRESSLAR,	
Are Chromæsthesias Variable?	632-646
BEATRICE EDGEHILL,	
On Time Judgments,	418-438
A. CASWELL ELLIS and MAUD M. SHIPE,	
A Study of the Accuracy of the Present Methods of Testing Fatigue,	496-509
JUSTUS GAULE,	
What is Life?	1-10
G. STANLEY HALL,	
Note on Moon Fancies,	88-91
Child Study at Clark University,	96-106
G. STANLEY HALL and THEODATE L. SMITH,	
Reactions to Light and Darkness,	21-83
JAMES H. HYSLOP,	
Binocular Vision and the Problem of Knowledge,	306-323

JOSEPH JASTROW,	
The Status of the Subconscious,	343-353
V AUGUST KIRSCHMANN,	
Deception and Reality,	288-305
O. KUELPE,	
Ein Beitrag zur Experimentellen Ästhetik,	479-495
JAMES H. LEUBA,	
The State of Death: An Instance of Internal Adaptation,	397-409
ADOLPH MEYER,	
An Attempt at Analysis of the Neurotic Constitution,	354-367
MAX MEYER,	
Experimental Studies in the Psychology of Music,	456-478
YUJIRO MOTORA,	
A Study on the Conductivity of the Nervous System,	593-614
G. T. W. PATRICK,	
The Psychology of Football,	368-381
W. B. PILLSBURY,	
Attention Waves as a Means of Measuring Fatigue,	541-552
E. C. SANFORD,	
On the Guessing of Numbers,	647-665
H. C. STEVENS,	
The Plethysmographic Evidence for the Tridimensional Theory of Feeling,	13-20
EDGAR JAMES SWIFT,	
Studies in the Psychology and Physiology of Learning,	201-251
E. B. TITCHENER,	
A Plea for Summaries and Indexes,	84-87
(See also note by Professor Joseph Jastrow and reply by Professor Titchener),	253
Class Experiments and Demonstration Apparatus,	439-455
MARGARET FLOY WASHBURN,	
The Genetic Function of Movement and Organic Sensa- tions for Social Consciousness,	337-342
GUY MONTROSE WHIPPLE,	
A Compressed Air Device for Acoustic and General Lab- oratory Work,	107-112
Studies in Pitch Discrimination,	553-573
LOUIS N. WILSON,	
Bibliography of the Published Writings of President G. Stanley Hall,	681-694
CORRESPONDENCE,	252-253
LITERATURE,	115-119; 254-264
Portrait of President G. Stanley Hall, facing page	267

THE AMERICAN JOURNAL OF PSYCHOLOGY

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No. 1.

WHAT IS LIFE?

LECTURE DELIVERED AT CLARK UNIVERSITY.

By JUSTUS GAULE,

Professor of Physiology, University of Zurich.

Not many years since tradition would have had a ready answer to this question. The whole Middle Ages is characterized by the tradition that life is a process caused in the body by the soul, or, in other words, by a living power. It was something supernatural that caused life, something eluding investigation, not subordinate to the laws of nature. In the meantime, mankind has been forced from this attitude toward the problem of life by necessity,—necessity, that is, the sufferings of mankind and the desire to heal or, at least to ameliorate them. Out of this sympathy and the wish to heal disease medical science has arisen. It first began by collecting facts concerning everything known that would alleviate pain, but, in proportion as this store of experience grew, in proportion as the attempt was made to systematize this knowledge, in other words in proportion as the art of healing began to be taught as a science, it was no longer possible to ignore the fact that disease is a change in the processes of life, and that it was only possible to recognize the changes when it was known what had been changed, that is, what life is. Thus, Physiology, the study of life, came to form the foundation of medical science. This Physiology has already developed some conceptions of life as a scientifically recognizable process. It is in these conceptions of life that I wish to introduce certain modifications and it is of these that I wish to speak to you today. My modifications are dictated by the apprehension that the now prevalent ideas concerning the phenomena of life

will not lead us to the point where we can obtain the desired knowledge of the condition of living organisms. They cannot form the basis of medical science because they do not represent the reality.

There is, for instance, the conception you will find incorporated in most text books of physiology that living organisms are comparable to machines. Just as the machine develops energy out of the materials furnished and converts it into work, so does the living being. At the last Physiological Congress in Turin I protested against this conception, and called attention to the fact that an essential difference between the machine and the living being consists in the fact that in the former the structure remains unchanged whereas in the latter the structure, that is, the body, changes. All experience, especially that of the physiologists, indicates that the difference between the machine and the living being is, that the former converts the calorics of combustible materials directly into energy; the latter, on the contrary, first makes the food a part of the body, while the tissues of the body in their turn decompose into simpler combinations thus developing energy. In a certain sense life consists of two processes, a constructive and a destructive, a truth that first occurred to Claude Bernard who says, "*la vie c'est la création et la mort.*"

Connected with this is a second modification of opinion that I would suggest. You will frequently find the living organism compared to a state consisting of cells in which every individual cell carries on a separate independent existence, takes its nourishment from the common store according to its need, and converts the calorics of the same into energy in compliance with its own organization. I do not admit that the cells are independent of one another. It is not so much that the term "*Zellen Staat*" is misleading, for in the state individuals are not so independent of one another as appears at first thought. It is the inferences that have been drawn from this term that make it misleading. Each cell, like each individual, has been considered equal to the others in respect to its food. Just as each individual needs a certain amount of carbohydrates, proteids and fats, so each cell takes its carbohydrates, proteids and fats from the supply in the blood and changes them into the final products of the metabolic processes. This is, in my opinion, a false inference. Each cell requires for the work it has to accomplish and, what amounts to the same thing, for the maintenance of its structure quite specific substances. They receive these substances from the other cells that have produced them through the medium of the blood or the nerves. The whole organism resembles a chemical laboratory with as many apartments as there are organs or glands.

The substances produced in each apartment are those needed in others either for their construction or for their work. I first had occasion to convince myself of this dependence of one organ upon another in a series of experiments to which I gave the name Experiments in the Trophic Functions in accordance with the usage of other investigators.

Fig. 1 shows what occurs in the *Biceps* and *Psoas* of the rabbit after the *ganglion cervicis infimum nervi sympathici* has been irritated. At *aa* a number of the muscular fibers have been changed while others near by are normal. These muscular fibers are torn and larger than the normal fibers, their contents have disintegrated into lumps and are stained dark with haematoxylin, while the normal fibers are red with eosin. Immediately after the experiment, the former fibers seem to be laden with a white inorganic substance that stands out distinctly against the red bottom of the groove lying between the two ends of the muscle. In the groove one sees swollen blood vessels, blood outside the blood vessels and numerous connective tissue cells and nerves. The only explanation that I could find at first for such a change in the tissues was, there were certain substances necessary for the normal construction of the muscle and that, in consequence of the irritation of the ganglion, it was receiving either too much or too little of the one or the other. Either the nerves or the blood vessels must have brought them here, the fact that the nerves showed changes in their structure spoke for the one hypothesis. Or the muscles may have received their materials from the blood and only the capacity to assimilate them from the nerve is lost, it acting in the character of a ferment. The swollen blood vessels and the circumstance that the alteration occurred subsequent to the irritation of the *ganglion sympathici*, that, as we know, contains the nerves of the blood vessels, seemed to favor the latter hypothesis.

All cells are dependent upon substances produced by other cells for their building material. Whether the nerves or the blood form the means of transit by which producers and consumers are connected, they form a unit. If the place of production is destroyed, or if the means of connection is interrupted, the consumer is also affected.

But my trophic investigations not only led me to a knowledge of this unity, but through them I gradually came to realize that the organism is in a state of ceaseless inner change quite independent of the experiments one makes upon it, or the special surroundings and conditions under which one observes it. Allow me to put this down as the third point in the change of front that I suggest, and to formulate it thus: Life is a continual change of the organism, that is influenced in-

deed by its surroundings but is not directly called forth by them. I gained this insight in the following way. While I was making test experiments to control the above mentioned observations, I made preparations of the muscles of animals that had in no way been operated upon and had been killed with chloroform. I discovered here similar changes to those in the operated animals. To be sure the changes were much less extensive than those in the operated animals, thus betraying the influence of the experiment, but what could call forth such changes in a normal animal? The first thought was, perhaps, that these are not changes but peculiarities of the muscles, but further investigation proved that at other times the muscles show none of these changes and were like those we consider normal. Thus there are changes, changes called forth by an experiment on the *nervus sympathicus* but that may also occur in the muscles of a normal animal, where no experiment has been made. I had to ask myself what these changes could signify? Then I observed some peculiar places in the skin-muscles of some rabbits that had not been operated upon. Microscopic investigation showed changes in the muscular fibers, in the blood vessels, in the connective tissue and in the nerves similar to those I had already seen in the *Biceps* and *Psoas* and have described above. These changes, however, were more circumscribed and much less extensive than those after an experiment. Their size is generally that of a pea. Fig. 2 is the photograph of such a change. The muscular fibers are torn for about 5 m. m., and the thickened ends, filled with coagulated matter, surround the intervening depression like a wall. Imbedded in the wall is the white substance already mentioned, that becomes whiter when treated with oxalic acid and dissolves in hydrochloric acid, thus indicating the presence of some lime compound. A medullated nerve is always to be found passing through the hollow and when the preparation is treated with perosmic acid, the nerve outside the groove is found to be stained black as usual. This medullated nerve, however, loses its affinity for perosmic acid at at least one point in the groove and during the remainder of its course therein it resembles a degenerated nerve. The white substance or myelin consists of blackened granules in the segments or cells as in Fig. 2. The blood vessels in the hollow are very remarkable. One finds regularly such an object as that at Fig. 2, b, namely a vein, to judge from the character of the walls, that is distended up to a semilunar object that abruptly checks the distension. Such an object is a familiar one to those who have made injections of blood vessels. It occurs when the injected fluid flows backward and is retained by the valves of the vein. Does the blood flow backward here? And what is the

connection between this process and the loss of myelin in the nerve, the tearing of the muscular fibers, the filling of the connective tissue with cells and the precipitation of a substance containing lime? The cause of this was not an experiment, the changes were called forth in the course of the undisturbed inner life of the animal. This can only be the result of a reconstruction taking place in the organism; this reconstructive process is so exaggerated at certain points that the function of the muscle is disturbed. The places are circumscribed so that the general life of the animal is not threatened. They may not affect the animal otherwise than in the form of growing pains that make us uncomfortable in youth. But these places are there; they are irrefutable witnesses of a process going on in the interior of the organism without external incitement. What is this process?

I have studied chiefly the frog for the changes taking place periodically in an organism, and have published the results in my paper "Die Veränderungen des Froschorganismus (*R. esculenta*), während des Jahres." The method by means of which I made these observations was to weigh the single organs and, in order to be independent of the varying size of the frogs, I estimated in each case the relative weight of the organ to the weight of the body. The organism of the frog is especially adapted for these observations because of the hunger period during the winter months. During this time no food is taken and a minimum of work is done, thus, if during this period the relative weights of the organs to one another vary, this can only happen by one organ losing while another gains, that is, one organ is reconstructed at the expense of another or from the material stored in another. The curves constructed on the basis of these observations and published in the above mentioned paper show that the relative weight of the sexual organs increases during the fasting period. This can only occur at the expense of the other organs and the reconstruction of the cells of organs into sexual products must take place within the living frog, during the hunger period, at a time when no external influence is affecting it and when it is quite oblivious to its surroundings. Miescher showed that the muscles of the salmon supply the materials for the sexual products. I further weighed the muscles of rabbits and found that their weight alternately increases and decreases. A report of these investigations was given at the Physiological Congress in Bern. On weighing the testes of the rabbit in the same way, I found similar variations of weight, betraying a similar relation of muscles to sexual products in the rabbit as in the salmon. The weight curve of the *M. gastrocnemius* of *R. esculenta* reproduced in the above mentioned paper disclosed a change here

also during the period of growth of the sexual organs. Now the second point in the reforms suggested leads one to think that it is not two organs only that are involved in this reconstruction. It is not simply that one organ supplies the material from which the other builds its cells. The whole organism is a unit, the life process is a unit, hence if one organ is changed all the others must change also, if sexual products are being formed then the whole organism must reconstruct itself. Hence it is that the curves of all the organs investigated of the frog also show a change in the relative weights in the course of the sexual period, that is, of the year. This is chiefly evident in the liver curves, the most important and largest organ of metabolism. The differences between the livers of the two sexes in the frog (*R. esculenta*) show what part the sexual organs play in this metabolism.

In the further course of this study of the changes in the organism that occur in connection with the reconstruction of the cells of the organs and the construction of the sexual products, I noticed something that seemed at first quite unexplainable. A more careful inspection of the curves makes it obvious that the growth of the sexual products is not continuous. The curves of the ovaries descend in March, those of the oviducts and the testes in February. The curves of the liver and the muscles on the contrary ascend in February. The opposite of what we found taking place in the other months occurs here, the sexual organs are not developed at the expense of the other organs but these at the cost of the sexual organs.

Now within two days in the month of February this year I discovered 10 frogs (*R. esculenta*), 9 females and 1 male, that had either none or very small sexual organs and this was the case without exception with all the frogs I examined during these two days. At the same time there was an exceptional increase in the size of the liver. Again, the difference in sex was especially noticeable in these enlarged livers.

Figs. 3 and 4 are photographs of microscopic preparations of these livers, the one of the male the other of the female frog in which the sexual products had so far disappeared that it was difficult to find the place where they had been attached. One sees a marked difference in them. In Fig. 3 the liver contains large groups of cells between the tubuli, and in Fig. 4 it contains nothing of the kind. On the other hand the liver cells themselves in Fig. 4 are much larger and occasionally there are small clumps of pigment cells between them. The large groups of cells characteristic of the female liver betray some connection with the blood, for they contain red blood corpuscles at various stages of development. Thus when testes and ova-

ries lose material, both deliver it to the same organ. The differences in the sexual structures disappear so far as concerns the individual. Yet the substances from the male and female organs evidently play a different part in the metabolism of the liver, the material of the ovaries passing into special groups of cells in which it is probably transformed into blood, the material of the testes into liver cells. Both materials pass into cellular elements of the producing organism but not into the same kind. I give these two illustrations because they show very distinctly the connection between the sexual and the individual structures, but not only on this account. The restless mill of change through which the organism is continually passing under the influence of cosmic forces finds its illustration here. What is happening here on a large scale is constantly occurring in the organism on a smaller one. How can such an inversion of processes take place in the organism? The fact of such an inversion we must accept for the curves show that before and after this period the sexual products are built up at the cost of the rest of the body and I could complement these curves with innumerable observations made in the course of other work. Something must happen just at this time in the month of February that causes an inversion of the processes going on before and after. This something influences the whole month of February as the curves for this month prove, but it is most evident on certain days to which those belong in which I studied the frogs this year. What can this something be? I expected that the whole sexual period, the maturing of the sexual products from one spawning to the next, was coincident with the cosmic period, the year. I discovered in other cases the coincidence of living processes with a cosmic period. At certain times the fat bodies of *R. esculenta* disappear during the night and are rebuilt during the day. This is an adaptation to the day period.

In Turin I showed how, in my study of the frog's blood, I had found an adaptation to the monthly period in the varying number of the blood corpuscles. How is it possible that the cosmic forces obtain an influence in the formation of the cells of the living organism? This can only be when the forces that cause them, the cosmic forces, also influence the fundamental processes of life. Nor is this very strange. An organism living in the world, must after all to some extent become adjusted to the mightiest forces governing the world.

What are these forces? Heat, light, electricity—their periods probably correspond to the cosmic periods mentioned. Now a living organism is under the influence of all these forces simultaneously. Something that, like the formation of the sexual products, requires an annual period for its

accomplishment, will at the same time pass through the daily and monthly periods. This building up occurs at the expense of the other tissues of the organism. The other tissues also pass through a periodic evolution in which a maximum and minimum are attained. Their periods must therefore have an influence on the formation of the sexual products. Now the periods are marked by the alternate approach of the organ to a maximum or minimum, and when these organs are in contra position to the sexual organs, while the sexual products are growing at their expense, then, when they approach a maximum a falling off in weight will be noticeable in the sexual organs. In most months this falling off will be obscured in the change of influence that the various organs exert over the sexual products. But the curves that I published last year at Turin concerning the number of blood corpuscles show that the monthly variations are very different in size, that is, the influence of the cosmic force upon the formation of the blood corpuscles is very different in different months. Hence, it is possible that in February a cosmic force of monthly periodicity may obtain such an influence over the organism of the frog that the sexual organs decrease while other organs, above all the liver, increases.

Hence the condition of the organism is at every moment dependent upon the cosmic forces affecting it, the mill of change that destroys cells and builds new ones is unceasingly active. Only when we have come to know the periods of the cosmic forces and their exact influence, can we say, that we may expect to see this or that in an organ. But are there not such representations of organs made without this knowledge? The text books of Anatomy, Histology and Physiology and other sciences represent and describe these organs as if they were unchangeable and of a quite definite structure. The most exact copy of a preparation of an organ can only possess the value of an instantaneous photograph of a transitory condition. Then one must remember that those that give these illustrations are not only photographers but also investigators. Among all the details the investigator selects the prominent and most frequently recurring features and the pictures they give are, to a certain extent, the *résumé* of a number of conditions.

Now I come to the last point to which I wish to call attention. Life in the living being is a continual process of reconstruction. In doing this it adapts this being to the world, it takes place under the influence of the cosmic forces. But to sustain life it must adapt the living being to its environment. Science has concerned itself until now almost wholly with this adaptation. This can only take place in the recurrence of the

secondary wave on the primary wave crest but it must also lead to a re-formation of the living being, if the propositions I have suggested are to be useful. Can we perceive anything of this re-formation? At present, and as long as the reconstruction under the influence of the cosmic forces is so little known, we can detect it only when the change of environment is great and very rapid. Then the inner change will be so significant that we can attribute it only to the change of environment. A year ago I was placed under such varied conditions of life that the changes that took place in my blood could be attributed only to these external variations in the surroundings. This was during my balloon ascents. In the first I reached an altitude of 5,300 meters and at 4,700 meters the number of blood corpuscles was counted and the unusual number of 8,800,000 was found. Since my companions also had a very high number of corpuscles I determined on my second ascent to make preparations of my blood in order, if possible, to discover the changes in it that might explain such an increase. The second ascent took us to the altitude of 4,200 meters. The number of my blood corpuscles was somewhat less than before, still 8,080,000 was an enormous increase over the number I had counted only three hours before in my laboratory. Fig. 5 is a photograph of the preparation made of my blood at 4,200 meters. Many of the blood corpuscles up there had nuclei and down below they had none. That the red blood corpuscles of man contain no nuclei has become an axiom of the histologist. These nucleated blood corpuscles throw some light on the manner of increase of the blood corpuscles, for nuclei are an indication of dividing, that is, multiplying cells. It shows us also that the blood adapts itself to the changed conditions in high altitudes by changing its structure, for the cells take the place of the structure of the machine. In so rapid a change we have not to consider a simple development of force between isolated molecules in a solution, no, we have a change taking place in complicated blood corpuscles. Just as the organism adapts itself to the world, the inner mill remodelling the cells under the influence of the cosmic forces, so it also adapts itself to the changed environment, the cells becoming different. We are ever under this double influence. The one process of ceaseless movement, by which the cells of the organism and the sexual products are formed, must combine with the other by which the cells sustain their life and that of the organism in opposition to the forces of the environment. The cells disappear again as quickly as they were formed. After my descent the number of my blood corpuscles was only 5,600,00. Fig. 6 is the photograph of a preparation of my blood then. The nuclei have quite disappeared. This case of adaptation is wonder-

fully instructive. It shows all the difference between the organism and the machine, the latter has to do with the development of power, the former with a change of structure. It also shows us that we are at the beginning of a new period of knowledge in which we shall be obliged to follow the state of the organism in connection with the conditions under which it sustains its life even to the cells. This period introduces new problems, it also promises us new fruits among which—if I overlook the effect upon medical science—I count, as not the least, a new conception of evolution.

EXPLANATION OF PLATE.

Fig. 1. Photograph of cross section of a skin muscle with trophic change. The unchanged fibers a a are stained red, those with trophic changes are stained from blue to dark blue and black. Section in Canada balsam.

Fig. 2. Photograph of a trophic change in the skin muscle. The muscle is spread out and treated with Os O₄ and laid in glycerine. a a The torn and changed fibers form a ridge or wall. b The nerve stained with Os O₄, b' point where the medullated sheath is no longer blackened. c The vein, c' the enlarged vein with the valve.

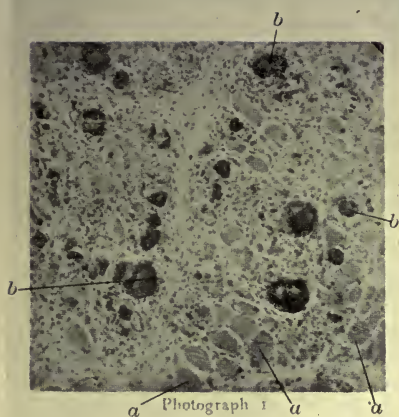
Fig. 3. Photograph of a section of the liver from a *R. esculenta* female after the disappearance of the ovaries. a-a Liver cells. b-b Large groups of cells between the liver cells.

Fig. 4. Photograph of a section of the liver from a *R. esculenta* male after the disappearance of the testes. a-a Liver cells. b-b Small groups of pigment cells.

Fig. 5. Photograph of blood at an altitude of 4,200 meters. Blood fixed in the balloon three hours after ascent. The nuclei are dark blue sharply defined spots. In some instances it is a mere point, at others it is larger. Highly magnified.

Fig. 6. Photograph of blood after the return from the balloon ascent. The blood corpuscles are rich in hæmoglobin, are evenly stained and no differentiation is discernible.

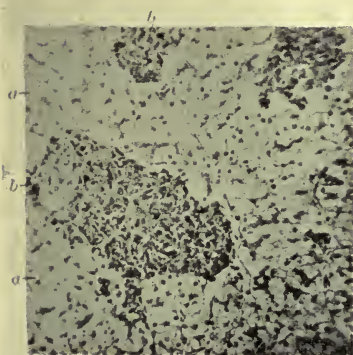
As the corpuscles are stained with eosin, which makes little impression on a photographic plate, they appear like shadows.



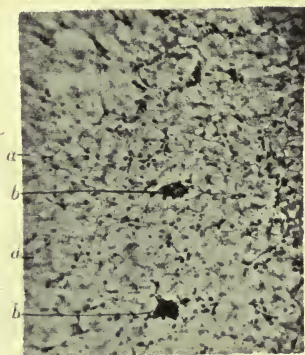
Photograph 1



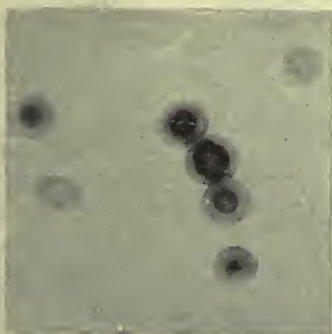
Photograph 2



Photograph 3



Photograph 4



Photograph 5



Photograph 6

THE PLETHYSMOGRAPHIC EVIDENCE FOR THE TRIDIMENSIONAL THEORY OF FEELING.¹

By H. C. STEVENS.

The controversy which, at present, exists in Experimental Psychology, between the adherents of the pleasantness—unpleasantness² theory of feeling and those of the tridimensional theory, needs a recapitulation of its arguments *pro* and *contra*. In 1896, Wundt published the tridimensional theory of feeling in the *Grundriss der Psychologie*; another exposition was published in the *Vorlesungen*, 3rd ed., 1897. In 1899 Titchener³ published a criticism of Wundt's theory. This, Wundt⁴ replied to at some length, in the same year. In the introduction to his article, Titchener points out that in the absence of experimental results which bear on the discussion, his arguments are based on introspection and general reasoning. But since the lack of experimental results affects Wundt as well, then, by inference, his theory rests on the same foundation as Titchener's arguments. The experiments of O. Vogt might seem to support the Wundtian theory; they are, however, not admissible since (1) the results do not agree with Wundt's; (2) the method of experimentation has not been confirmed. There are two specific objections to the tridimensional theory and some negative observations.

(1) Since feelings are limited by maximal contrasts, no pairs of feelings would be true feelings unless they exhibited this peculiarity. E—D and S—R do not; D is or may be merely the absence of E; R is or may be merely the absence of S. Neither D nor R is a truly active feeling, and therefore cannot be the maximal opposite of E and S. (2) Wundt gives two different accounts of the way in which the chief directions of feeling are related to other mental processes. (a) In the *Vorlesungen*, P—U correspond to the qualitative aspect of sensation; E—D, to the intensive aspect; and S—R, to the temporal aspect.

¹From the Psychological Seminary of Cornell University.

²Throughout this article, I shall indicate respectively Pleasantness-Unpleasantness, Excitement-Depression, and Strain-Relaxation by the letters P-U, E-D, and S-R.

³Zur Kritik der Wundt'schen Gefühlslehre. *Zeitschrift für Psych.*, 19, 321.

⁴Bemerkungen zur Theorie der Gefühle. *Phil. Stud.*, 15, 149.

Now, if quality, intensity and duration are represented in the directions of feeling, ought not our spatial experience also to have a direction of feeling, *e. g.*, expansion and contraction? (*b*) In the *Grundriss*, there is a different explanation of the relations. If a feeling modifies a present state of consciousness, it is P—U; if the feeling exerts a definite influence on a future state, it is E—D; if the feeling is determined in its peculiarity by a past state it is S—R. In the former case, the directions of feeling depend on the attributes of sensation; in the latter, on the temporal course of consciousness. (3) The third point consists of some introspective observations. A practised student observed his affective experiences during the year; the result was that no emotive content, besides P—U, was observed which could not be definitely localized in a bodily organ.

Wundt replies to this criticism in detail. He first considers the statement that no experimental results were extant prior to the publication of his theory. He indicates several sources of such results. (*a*) He himself had pointed out, in the 4th edition of the *Grundzüge*, that P—U are mainly dependent on the common feelings; that, in the case of tones and colors, such terms as 'stimulating' and 'mild,' 'exciting' and 'depressing' are necessary to describe the attendant feelings. (*b*) There were also Mosso's older experiments with the balancing board and hydrosphygmograph, as well as those of Kiesow and Mentz with taste and acoustic stimulation. (*c*) In the present paper, Wundt finds corroboration of his theory in Lehmann's *Atlas of Plethysmographic Curves*. We shall return to this point later. (*d*) Wundt also returns to O. Vogt's results. This may be due to the opportunity which Wundt had had of seeing the method in use, during a visit of Vogt to Leipzig. The results are in essential agreement with his own. Vogt has a *hebende* and a depressing feeling, and S—R. There are, however, two differences. (i) Vogt has a feeling of activity (*Activitätsgefühl*) which is concomitant with the activity of the will. (ii) The types of feeling are not merely directions of feeling, but are themselves simple, indivisible feelings. In accounting for these differences, Wundt identifies the feeling of activity with E, probably in combination with S—R. The second point of difference, he thinks, is due to the small compass of stimulus effects that Vogt observed.

(1) To the first objection, that D—R are not active feelings and therefore not the maximal opposites of E and S, Wundt answers that they are *facts*, determined by introspection. (2) *a*. Spatiality is not one of the feeling directions, because it does not appear either in introspection or in expressive movements. *b*. As to the two references of the feeling directions, they are merely two ways of viewing feelings in relation to other mental

processes. On the one hand, feelings are related to the attributes of the sensation; on the other hand, they are related to its temporal course. These two relations are not mutually exclusive. (3) Wundt criticises the introspective observations, *a.* because introspection was used without experimental control, and *b.* because the statement of the result—that besides P—U, no other emotive content was found which could not be definitely localized in a bodily organ and was therefore a sensation or sensation complex—implies that feeling does not depend upon sensation. But this is not true.

So far, our purpose has been to summarize the arguments and replies of the two protagonists in the discussion, with a view to bringing out the state in which the question at issue now rests. The more immediate end of this essay, however, is to test the validity of the interpretation placed by Wundt upon certain of Lehmann's curves.

Up to 1899, the most complete and available experimental material for the study of feeling was afforded by Lehmann's¹ *Atlas of Plethysmographic Curves*. As already mentioned, Wundt² found substantial support for his theory in these curves; and, indeed, with the possible exception of Mentz' work, this was the first considerable experimental investigation of the problem of feeling. It is necessary to distinguish, in these curves, two distinct types of reactions. On the one hand, there are the simple, unequivocal reactions of P—U; on the other, there are complications of reactions, which are considerably involved. Wundt is concerned mainly with the latter, and, in his interpretations of them, differs radically from Lehmann. Lehmann thinks that the "resultant" curves are complications of P—U effects with strain-states (*Spannungszustände*) of the attention; the strain-states he believes to be sensation processes only, without any feeling character whatever. Wundt takes an opposite view. He says: "I do not, however, consider as such components, P—U on the one hand, and other states of consciousness not affective in character, on the other; but feeling components of different quality and expression, throughout."³ Wundt's procedure, then, in turning the resultant curves to account for his own theory, consists in finding curves, the pulse-characteristics of which agree with the

¹Leipzig, 1899.

²It should be pointed out that Wundt's draftsman has reproduced but poorly the curves of Lehmann's atlas. For example, Fig. 3 is 2 mm. shorter than the corresponding part of Lehmann's curve; and Fig. 1 is 1.5 mm. shorter; also, the reproductions of many individual pulses are grotesque.

³*Loc. cit.*, p. 157.

logically determined pulse-characteristics of the chief directions of feeling. Thus, the pulse of P, E and R is intensified; but P retards the rate; E does not change it; R accelerates it. Similarly, S, D and U weaken the pulse; while S retards the rate; D makes no change; U accelerates. Now, the purpose of this essay is to inquire how far the physical characteristics of the curves bear out the constructions that are put upon them; and that on the basis of Wundt's own pulse determinations.

Wundt quickly passes over curves of P—U. These reactions are well known. He finds, however, two plates, XXII and XXIII, which show the expressions of E—D unmixed, at least, with P—U. These plates give the results of experiments with a state of feeling which Lehmann was, at first, unable to explain. The observer P. L., "a strongly built grown man," gave a curve of small constant volume and small pulse. This reaction, Lehmann suspected, was not normal. After several attempts, it was evident that the state was proof against ammonia stimulation and the fright caused by a sudden noise, XXI B. C. It yielded, however, on another trial, XXI D., a small increase in volume to fright (contrary to the normal reaction), and a marked reaction to P, which was exceptional. These were the circumstances under which plates XXI, XXII and XXIII were obtained. That is, an abnormal state was present, the symptoms of which were constantly diminished arm-volume and small pulse.

After failing with more severe stimuli, Lehmann attempted to dissolve this state with "weak, pleasant and sufficiently varied stimuli," such as could not set up a psychical strain (*Spannung*.) He began, XXII A, with "some weak tuning fork tones." Two stimulations were given: (i) short; this yielded a plain increase in arm-volume; (ii) long: this yielded an equally plain decrease. The next three curves, however, B, C, D, showed certain common variations. At the very beginning of the curves, the arm-volume was large and the pulse high. Very soon the volume sank to a low level with small pulse. In XXII E, Lehmann touched the observer very softly on the ear; this was pleasant; a little later the volume increased, and maintained itself at a high level with large pulse. This reaction gave Lehmann the key to the explanation. He says:¹ "I now feel tolerably certain of my case; the relatively large volumes, with high pulse, were the indication of the normal balance of affection in which the observer happened to be when he did not expect a new experiment; but as soon as an experiment impended, the psychical strain (*Spannung*) returned, characterized by small arm-volume and low pulse. If

¹ *Die körperlichen Aeusserungen psychischer Zustände*, p. 81.

this be the correct view, then one would need only to wait a moment for the strain to cease, and the reaction would become normal again." Plate XXIII furnishes proof of the correctness of this hypothesis. In A, the stimulus was a loud noise, which produced some fright. The fright expressed itself by a small decrease in volume; after this, there was a distinct increase in volume above the original *niveau*, with increasingly larger pulse. Similar changes took place in the three other curves. With reference to this set of experiments, XXI—XXIII, Lehmann says:¹ "The main fact that emerges out of all these experiments with irregular results is, that the observer was only rarely in a normal balance of affection; while another state of feeling dominated throughout. That this foreign state was strain or expectation is not however completely proved; it is only a provisional hypothesis, if, at the same time, a very natural one." He says, further, that sure proof would be furnished, if a state of strain (subjectively certified to) could be induced, the symptoms of which were identical with the curves just noticed. Such curves are XXIV A, C, D. This evidence Lehmann considers conclusive, and in the light of these results he finds many other examples of strain in other observers. For example, XXV A, B, C, D.

Wundt calls these reactions relatively pure expressions of E. The symptoms of this state are a "sudden increase of arm-volume and of pulse intensity, without further noticeable change in the temporal course of the pulse."² The only curves in plates XXII and XXIII which answer this description, in part, are B, C, D, E XXII; A, B, C, D XXIII. The reactions in these curves agree with the two positive characteristics of E, *viz.*, the increase in arm-volume and the intensification of pulse. But these symptoms apply equally well to P and R. Also, the sudden rise is not discriminating, since the rise is by no means sudden in all cases; *e. g.*, XXII C, E and XXIII A, C. The *differentia*, therefore, of P, E, and R must depend on some other change in the pulse than intensification. According to Wundt's schema, this *differentia* will be temporal: retardation for P; constancy for E; and acceleration for R. These temporal changes may be accurately determined by comparing the average length of pulse in the first part of the reaction with the average length of pulse in the last part. The result of the measurements is given in the following Table:

No. of Pulses	Av. Length Lower Half	Av. Length Upper Half	Curve	Stimulus
3	4.3	5.6	XXII B Initial Rise	Spontaneous
5	4.4	5.0	" C After Stim.	8+16+21
9.5	4.0	5.2	" E " "	Touch on ear
4.5	4.4	5.4	XXIII A After Fright	Loud noise
3.5	4.2	6.4	" B " Stim.	7 x 14
4.0	6.7	6.3	" C " "	Tuning fork.

¹Lehmann: *ibid.*, p. 83.

²Wundt: *ibid.*, p. 156.

With but a single exception, the average length of pulse in the upper half of the reaction shows a decided lengthening—and therefore retardation—over that in the lower half. The reactions can, therefore, hardly stand for E, as Wundt says they do; on the basis of his own *differentiæ* they stand for P.

Wundt next considers the “resultant” curves. Of these, he says, “Lehmann has also pointed out these complications in many places in his work. But even alone, they force themselves upon one by a study of the pulse symptoms, on the one hand, and of the subjective facts, on the other.”¹ In support of this statement Wundt indicates an example of each. The subjective report is the phrase ‘*überraschender angenehmer Geruch of patschouli.*’ Wundt says of this: “I think that one may, without further evidence, read out of these words, in which the author characterized the psychical impression, the combination of E and P.”²

It may be true, on the subjective side, that E is present; but there is little evidence for it in the physiological expression of the curve itself, XLIV C. The reaction exhibits the sudden rise in volume and intensified pulse that characterize P, E and R. The *differentia* in favor of E is constancy of pulse during the reaction. If we resort to the previous test, the measurement of the lower half of the reaction gives an average length of pulse of 4.8 mm., against an average of 5.1 mm. for the upper half. With reference to the evidence from the pulse symptoms Wundt says: “Since the pulse symptoms of P and E are very similar to each other, except for the very plain retardation of pulse in the case of P, in this curve XLV, A, at least, the combined effects are different in the highest degree, so that with a definite increase of pulse intensity and arm-volume there is observed rather an acceleration of pulse than retardation.”³ The symptoms of this curve are sudden increase in volume, constant height and acceleration of pulse. Altogether, it is an anomalous reaction. It is neither P nor U; and the place for it in the tridimensional theory is unsatisfactory. These symptoms call for R. But R could not be present, unless its co-ordinate state, S, preceded. The initial curve, however, is normal; S may therefore be ruled out, and consequently R.

Thus far, Wundt has made three attempts to demonstrate E. First, with the pure curves of E; then, on the basis of introspective evidence; finally, in complication with P. He makes another attempt on the basis of a complication with U. The three plates which Lehmann exhibits as examples of the phy-

¹ Wundt: *ibid.*, p. 156, 157.

² Wundt: *ibid.*, 156, 157.

³ Wundt: *ibid.*, p. 157.

siological expressions of voluntary attention, Wundt construes as mixtures of E and U. The stimuli in these experiments are problems in multiplication. Wundt appeals to ordinary experience for proof of the uneasiness combined with U which arises from mental reckoning. E supposedly comes from mental activity, although nothing is said about that. These curves, and they are remarkably uniform, are characterized by Lehmann as follows: "A concentration of attention (thought) is immediately accompanied by a few rapid pulses, during which the volume shows a tendency to rise. Thereupon follow 4 to 8 pulses, during which the volume shows a tendency to sink; the length of these pulses is always greater than that of those immediately preceding, often even surpassing the norm. Finally, the volume rises with rapid pulse."¹ From this description, it is evident that it will be very difficult, on either theory, to explain the reactions as affective reactions. The symptoms of P—U are just the opposite of these reactions, *viz.*, rising arm-volume and retarded pulse, for P; and sinking volume and accelerated pulse for U. It is true that the tridimensional theory has affective states which correspond with these symptoms; but they are hardly intelligible in this connection. For example, rise in volume and accelerated pulse call for R, in Wundt's schema; while fall in volume and retarded pulse call for S. Since, however, these states are co-ordinate, the presence of the one presupposes that of the other; but not in reversed order, as in this case. S must precede R, as E must precede D. Therefore in this case R could not precede S, unless another S had preceded R. But there is no evidence for this, as the initial curve is normal in every respect.

Wundt seems curiously in error with regard to these curves. He describes them as follows:² "They show, as a rule, the decrease in arm-volume characteristic of U and occasionally, also, acceleration of pulse; but acceleration of pulse occurs with an increase in arm-volume, not with a diminution. Frequently, also, these different symptoms succeed each other; first, the arm-volume sinks with an increasing pulse rate; then, it rises, in accordance with the growing uneasiness which is wrought by the difficulty of mental reckoning." These statements, so far as they are intended to apply to plates XV, XVI, XVII, are inaccurate. The arm-volume never falls first; it rises, or tends to rise.

The demonstration of S—R is also attempted. The symptoms of these states (*Spannungszustände*) are described by

¹Lehmann: *ibid.*, p. 68.

²Wundt: *ibid.*, p. 157.

Lehmann as "continuously diminishing arm-volume with lessened intensity of pulse."¹ Wundt, however, sees in some of the curves still another symptom which he describes as a "retardation, therefore lengthening or at least . . . an unchanged magnitude of length of pulse."² The plates referred to as examples are XXIV and XXVI C. D. The decision of this point will depend upon the time relations of the curves themselves; and since Lehmann had already been led to consider the relation of the rate of pulse to S, we may use his measurements.

Curve	Period	Strain State	Period	Normal State.
XXII D, E.	e—f	5.2	i—m	5.1—5.2
XXII C	c—d	4.9	g—h	5.0
XXIII C	—	7.0	—	6.6
XXIV A, B	d—e	5.8—5.3	i—k	7.0
XXIV C, D	l—m	4.4	q—r	4.4

Lehmann's conclusion is this: "S does not cause a constant change in the frequency of heart rate. The length of pulse may be longer at one time, and again shorter than the norm. But it never varies greatly from it."³

Although Wundt cites plate XXVI in this connection, it need not be considered, as there is no evidence that a state of S is present.

The conclusion to this paper is, then, that Wundt's appeal to Lehmann's atlas, for evidence in support of the tridimensional theory, is unsuccessful. It is unsuccessful because (1) the examples of E do not show constancy of pulse rate during the reaction; because (2) the complication of E with P is not intelligible on the tridimensional theory; because (3) the introspective evidence of E is not borne out by the pulse characteristic of the curve; because (4) the complication of E with U is not intelligible on the tridimensional theory; and because (5) the retardation of pulse, in the case of S, is not borne out by measurements of the curves.⁴

¹ Lehmann: *ibid.*, p. 89.

² Wundt: *ibid.*, p. 158.

³ Lehmann: *ibid.*, p. 89.

⁴As will have been apparent, the aim of this article is in no way similar to that of the recent paper by R. Müller, on the applicability of the plethysmograph to the study of the affective processes (Z. f. Psych., 30, Heft 5 and 6). The purpose of Müller's article is to set forth the physiological factors involved in certain variations of blood-volume and blood-pressure, and also to criticise the adequacy of the plethysmographic method to the expression of these variations. If Müller's criticism is directed against any one person, that person is Lehmann. Our own attempt has been to show the invalidity of Wundt's interpretations of certain of Lehmann's curves, selected by him on the basis of his own *differentiae*.

REACTIONS TO LIGHT AND DARKNESS.¹

By G. STANLEY HALL and THEODATE L. SMITH.

Cox, in his mythology of Aryan nations, following the lead of Max Muller, traces not only the Greek epics but a large part of the Aryan myths to a solar origin. When to this fact is added the prominent rôle which the heavenly bodies assume in Indian folk tales, and the important part which light plays in the development of both plant life and animal organisms, the subject becomes most suggestive as a field of psychological research. Of the many problems which the subject presents, it is the aim of this paper to discuss only those which are directly concerned in psychic reactions, though incidentally some physiological effects are included in the returns. Of these, 427 have been tabulated, 312 of which are from normal school pupils of ages varying from eighteen to twenty-two with a very few above and below those ages, while 38 are from children of the 5th grade, of from ten to twelve years old. These cover a more limited range of topics and have been tabulated separately. 77 returns are from negroes, 28 of these being from a colored girls industrial school, the ages of the pupils ranging from ten to sixteen years, and 49 from a mixed colored school; 23 of these are males of ages varying from sixteen to twenty-seven; 26 are girls of ages sixteen to twenty, only two being above that age. No specific differences have appeared between the returns from white and colored pupils except such as can be directly referred to degree of educational opportunity. The figures given represent always the number of cases and not the number of papers, as the two do not always coincide. All answers which showed a lack of comprehension of the question have been excluded in tabulating results. For obvious reasons no classifications on sex lines have been attempted, and no definite age limits at which theories, fancies and sentiments were most prominent in mental life can be

¹Acknowledgments for returns are due to Miss Lillie A. Williams, New Jersey State Normal School; Miss Margaret K. Smith, State Normal and Training School, New Paltz; G. E. Partridge; Rev. Pitt Dillingham, Calhoun Colored School, Calhoun, Alabama; Miss Sadie J. Lime, Colored Girls Industrial School, Moorhead, Miss.; Dr. Anagnos, Perkins Institution for the Blind, South Boston. Mr. C. E. Brown also helped in tabulating the negro returns and looking up Indian myths.

worked out as the data are very vaguely given in the returns. The first topic of the syllabus has reference to reactions at dawn and to the fancies and feelings closely connected with it. It was as follows:

In studying the reactions of sense, mood and motion to different degrees of light, we need many experiences based on observation upon children, reminiscences of adults, and present feelings of young people and adults. In general, bright light increases activity of all kinds, and darkness tends to diminish it, but the items wanted are more special, as follows:

1. Reactions of feelings at dawn. Is its advent longed for on waking too early; is there ever any anxiety lest it should not come or is unduly delayed; any feeling that the sun makes a great effort or has great labor to move up over the horizon, to break through or drive off clouds, or to banish darkness as if it were its enemy; any feeling of dualism as if light and darkness contended or struggled with each other, or as if day and sunrise was a victory, as if ghosts or any other night fears were driven away? State fully any fancies, dreams or romances. What makes the sun rise; by what power?

In tabulating the returns on this topic, it appeared that out of 389 cases, 207 had experienced longings for dawn in health and 123 during illness, making a total of 330 or nearly 85%. Twenty-two of these answering the question, however, state that the feeling was only on occasion of some expected pleasure which morning was to bring. The character of the longing in the other cases, which varies from mere restlessness to a real light hunger is illustrated in the following extracts quoted from the returns.

F., 19. When I awake before dawn as I often do if I have anything on my mind, I feel an intense longing for daylight almost as if I were in bondage to darkness and the light would set me free.

F., 18. It sometimes seems as if the dawn would never come. (A quite frequent expression.)

F., 18. It sometimes seems as if daylight would never come although I never have any real fear lest it will not come.

F., 17. On my sixth birthday, I thought the dawn would never come. I was to go to Philadelphia to meet my father.

F., 19. As far back as I can remember, I have never wished for the dawn. I love the night and what it brings, darkness, rest, stillness, peace, pleasant dreams and a great overpowering calm. It has always been the same. I have longed for night. It soothes and lulls me and I love to lie awake and build air castles.

F., 18. Sometimes during a night of illness I have longed for dawn but I never remember longing for the dawn when I was in good health. Neither have I had any anxiety lest dawn should not come except when I have been ill or worried, then sometimes the hours seemed very long and dawn afar off.

F., 20. I have thought that dawn would never come but I always had an idea that it would come sometime.

F., 19. As a child I do not remember ever longing for day break except when there was some special reason such as sickness or anticipation and at such times the early morning hours have seemed unduly delayed. But about a year ago, when visiting, a child of three slept with me for several nights. Regularly at dawn this child would

awake and begin at once to romp to play and was not satisfied until every one within reach was awakened also.

F., 19. When I was about seven I sometimes fancied that the dawn would never come; then I used to wonder what would happen if the sun would never come again; tried to imagine how we would live if it were always dark. These fears always disappeared with the night.

F., 19. When about six to ten years of age, I thought that when the light came everything was bright and happy and I always felt like jumping and skipping about.

F., 20. I do not long for the advent of dawn on waking too early but always desire to sleep longer and wish that it would not come.

F., 22. As far back as I can remember I have had a very peculiar feeling when I awakened before dawn. The feeling is as if all my nerves were tingling with an intense longing for light.

Only 61 out of 312 had felt any anxiety lest dawn should be delayed and 46 of these cases occurred during illness. Of the remaining fifteen cases, five can be traced to a specific cause and the language of the remaining ten leaves it somewhat doubtful as to whether the anxiety was real or a mere figure of speech. 184 state positively that they never experienced any such feeling and 79 leave the question unanswered. In this case and throughout the paper these failures to answer cannot be considered as negatives, since they represent, as stated in many instances, failures to recall distinctly, lack of observation or thought on the topic and, in some cases, a failure to comprehend the question. Out of 338 cases, 78 had, at times, experienced a fear lest morning should not come; 32 of these being ascribed to illness and the remaining 49 to some period of childhood. 267, nearly 80%, stated that they had never experienced anything of the kind and 80 left the question unanswered. In these returns it is to be noted that only 5% of the total number of whites answering the question had ever experienced the fear while in health, and that all these cases are referred to childhood, while over 37% of the colored state the existence of the fear without time reference. The character of these feelings is illustrated in the following quotations.

F., 19. Once after a great thunder storm in the night I was afraid lest morning should not come. I was about seven years old and I thought maybe we would have darkness for a long time.

F., 18. I never felt any anxiety lest the sun should not come. I never thought about it. I always felt that the sun glides up very gracefully without the least effort. Sometimes it seems to have a struggle to break through the clouds.

F., 19. Many times on waking early in the morning I have become very impatient and gotten up to see the time, as I think daylight will never come. When a child I would often lie in bed early in the morning and watch the sun rise. It seemed to me to have great difficulty and to be struggling with the clouds. The sun always appears to me as if it were struggling to banish darkness.

F., 21. Many times on waking early in the morning I have gone to the window to see if the sun had yet risen and wondered what was

delaying it. I have imagined that the sun had great difficulty to move up over the horizon. I often thought it stopped for a few moments of rest and then proceeded on its journey.

F., 19. I recalled the chapter where Joshua commanded the sun to stand still and was afraid the sun might be delayed in rising.

F., 17. I can remember feeling an anxiety lest the dawn should not come when I was about six years old.

F., 19. I always took it for granted that the dawn would come.

FEELING THAT THE SUN MAKES AN EFFORT.

Of 312 answering the syllabus 102 had once felt or still had the feeling that the sun makes an effort or has labor to move up over the horizon, break through clouds or banish darkness; 106 had never had the feeling and 104 gave no answer—an almost equal division of results. 102, 27%, had experienced a feeling of struggle between light and darkness, usually accompanied by the feeling that the sun was a victor; 202, 50%, had no such feeling and 85 gave no answer.

F., 18. In watching the sun rise I have often felt that it made an effort to get through the clouds and above the horizon. When it did finally appear I would draw a breath of relief.

F., 19. The sun seems to me to glide from behind the hills without any effort.

F., 20. It always seems to me that the sun moves with ease and as if darkness and clouds were dispelled easily.

F., When I was about six years old I used to think that it was an effort for the sun to get up in winter. I thought that the sun liked to lie in bed when it was cold. I thought that there was some force which pushed the sun over the horizon and then the sun was able to proceed without further assistance.

F., 19. I have no feeling that the sun makes an effort but rather that the clouds make way for it.

F., 19. It always seemed as if the sun came up as easily as a balloon would rise.

F., 17. I never felt that the sun made an effort to rise but rather that he was so strong and mighty that he shone right on whether clouds came before him or not.

F., 20. It never seemed to me that the sun made the least effort in rising for I associated its progress with that of a swiftly moving winged creature whose face gave out the light; but as the darkness never seemed embodied, the idea of strife between light and darkness is a figure which never entered my mind.

F., 19. As I watch the sun move up over the horizon, I always have a feeling that it is being slowly pushed up by something. It seems to me that it is trying to push away the clouds. After a shower, it has seemed to me as if the sun made a great effort to banish the darkness.

F., 20. When I watch the sun coming over the horizon I always imagine that some one is behind it in order to push it over the horizon.

F., 19. I never had any feeling that the sun makes a great effort to move up over the horizon unless it has to break through clouds, and then I imagine it is like a mad person rushing right through all obstacles and then coming out victorious with smiling face.

F., 18. Looking at the sunrise I have thought that something was holding the sun down so that it could not rise (13 yrs.).

F., 19. The house in which I live faces the east and directly in front of it is a rather thick wood. As the sun rose I often thought it was making an effort to get away from the branches of the trees.

F., 17. I have had a feeling that the sun made an effort to rise above the clouds but the feeling was not very pronounced. When the sun did come up I experienced a feeling of relief.

I have never felt that the sun made an effort to get up through the clouds but I have often felt that the moon did.

F., 19. When looking at the sun rising I often, as a child, and even now have the feeling that it has to make a great effort to get over the horizon. I have had a similar experience with the moon.

F., 17. I knew that the sun was stronger than the clouds and I used to feel happy when he emerged from them.

Feeling of Dualism Between Light and Darkness.

I thought that darkness and light were kings of two different realms and that at dawn they struggled for possession, when the king of darkness finding that he would soon be overcome fled in great haste from the scene of conflict (9 yrs.). I had an indistinct notion of conflict between light and darkness. When I saw the sun rising, I thought it had been asleep during the night and was just getting up.

I have watched very few sunrises and cannot remember ever feeling that the sun made an effort or that it was hard work for it to get over the hills or clouds. I never had a feeling of any conflict.

I never felt that there was any struggle further than that the sun was chasing the darkness before it.

I thought of the darkness as something that might gather me up, then the glorious sun chased the darkness away.

The only fancy that I ever had about the sun was about its rising on a cloudy day. Then I thought it was trying to struggle away from the clouds and when it had, at last, succeeded it was laughing because of the victory it had gained.

I have often thought that the clouds were simply obstacles in the sun's way and that he gained a great victory when he had passed through one and thus had courage to struggle through the next cloud when he came to it.

F., 18. As a very little child I thought that light and darkness struggled continually, but when I was seven or eight years old my opinions changed and I began to think that light and darkness were brother and sister, one helping the other all the time and when light was tired darkness stepped in and put her to bed.

F., 19. It did seem to me like a conflict between light and darkness in which I felt the sun and light would conquer and drive away the darkness. I have noticed the feeling that there was a victory over the clouds more with the moon than with the sun.

F., 19. I have always thought that light was contending with darkness to see which one could banish the other. I have imagined that as soon as it was dark, ghosts came out and held conversation with one another but that as soon as it grew light they vanished and hid themselves in church yards and other dreary places. As I became older I lost all such fancies.

F., 19. I never had any feeling of dualism as if light and darkness contended with each other; but I do sometimes have a feeling as if sunrise and day were victorious over darkness and night.

F., 17. I have never had the feeling that light and darkness contended with each other or that day and sunrise were a victory. Yet I like poems that express such sentiments.

Of the 427 cases tabulated 154, over 36%, give no sunrise

theories or fancies except such as were directly derived from the study of geography, without elaboration. This estimate, however, includes all the cases of "cannot remember," "do not recall," and failure to answer, as well as the direct negatives, so that it is probably too large to be representative. Seven children and six adults had never seen a sunrise. The negative in these cases is probably an absolute one.

Of ideas occurring frequently the following are typical. God makes the sun rise, 43 times; it is pushed or pulled up by some person or power, 38 times, and rises by its own power, 42 times. These three ideas are so varied and elaborated by individual fancy that the number of forms in which they are embodied seems limited only by the number of individuals. In the returns from children, three gave heat as the cause of the sunrise; two, clouds and two pressure of daylight. A boy of twelve said, "the light of the sun gives it strength to come up in the sky."

No line of demarcation can be drawn between sunrise theories and fancies as the element of fancy enters very largely into all the theories, but the variety and richness of fancy and the tendency of the normal childish mind to seek some explanation of natural phenomena is fully illustrated in the succeeding pages.

F., 18. When a small child I had an idea that the sunrise was God lifting up his curtains and the sun helped to hold them up.

F., 8. My theory of sunrise was that it just came out of the earth. I still always think of it in that way though I no longer believe it.

F., 19. I thought the sun got up in the morning just as people do.

F., 8. I thought there was some force which helped or pushed the sun over the horizon and then the sun was able to proceed without further assistance.

F., 20. Before I was seven I often used to wonder what made the sun rise. I used to think that God must get back of the sun and push it up in the sky.

F., 14. The sun never seemed to me to be propelled by any force but simply rose in the horizon and moved across the heavens by its own free will just as a person strolls leisurely along a pleasant pathway.

F., 17. It seemed to me that the sun rose as a bird might fly upward by its own power.

F., 18. I thought there must be a sort of machine inside the sun by which it could move.

F., 20. I always thought of the sun as having the power of voluntary action.

F., 19. When a child of about five, I imagined that God made the sun rise. I never thought where it went at night, but thought of it as some great person showing superiority. I imagined it to be a proud person with golden hair and that the clouds and darkness were glad to flee from as they were afraid that they would melt.

F., 18. The thought what makes the sun rise, "by what power," never entered my head.

I don't think I ever thought what power made the sun rise.

I just thought of it as getting out of bed and fighting.

M., 19. I never formulated any childish theory as to what made the sun rise, it seemed perfectly natural and the idea never occurred to me.

Some theories of sunrise, given as reminiscences, are :

F., 18. I felt that some one was pushing the sun up and that the sun was a heavy load.

F., 17. I used to think that God had a long pole and pushed the sun up to us when he wanted us to get up and up from the little Chinese boys and girls when he wanted them to go to bed. I used to try to think and reason out why the sun did n't go out when it came up from the sea, just as a lighted match did when I stuck it in the water.

F., 19. I never had any theories of what made the sun rise, I thought that it simply rose of itself without any help.

F., 18. I used to think that the sun rose just as we do because it was morning. I never associated sunrise and early dawn together.

F., 19. When I was a small child I had a fancy that the sun was a large ball on a string and God pulled it up. When I grew older it seemed more like a smiling person, and as I had been told that the earth was very large and the sun spent the night on the opposite side from where we lived, I, of course, thought sunrise was the sun coming back to us, but not as I think of it now, always as a person.

F., 18. When I was a child of six or seven years old, I could offer no explanation other than that the sun was a large moving ball of fire, but when I was about four or five years old I thought that the moon was God's big eye and the stars were his candles.

F., 19. I thought there was a man pushing the sun upward and that darkness flew in front of him so swiftly that by the time the sun was ready to sink, the darkness had come around the earth and was closely followed by the sun until wearied, when the sun would again overtake him.

F., 18. I thought of the sun as pulling himself up like the weights of an old-fashioned clock.

F., 20. Sunrise is occasioned by the shooting of the cannon.

F., 17. I have sometimes thought that there was a powerful engine behind the sun which pushed it.

F., 18. I always think of the sun as a large horse prancing up over the hills.

F., 20. As a child I never had a thought of what made the sun rise.

M., 12. The heat is so hot it makes the sun rise; the power is that the heat is so strong it comes up.

M., 13. What makes the sun rise is that it has power just like a man or boy that has muscles. The power that makes the sun rise is the mind's power.

F., 12. The sun rises because it wants to show us light.

F., 11. I think air makes the sun rise. The earth causes it to come up in the sky.

F., 18. The sun was always running races with me to see who would get dressed first.

F., 10. I think clouds make the sun rise.

As a child I always thought that there was a beautiful fairy with golden hair, blue eyes and a long flowing robe who was the mother of the sun and every morning she gave the sun a bath and dressed it in a beautiful golden dress and sent it out for a walk. When we had cloudy days I used to think that the sun was ill and could not go out for a walk.

I used to think the sun was a great ball which simply rolled about

in the heavens and explained its rising and setting that way. As a child I thought that the sun was run by machinery. I thought that the sun floated along in the air like a balloon and God sent it.

When I was very young, about four years I think, I thought the sun was God. When it was dark I thought God was asleep.

I always pictured the dawn as an airy graceful girl in glistening robes, but somehow with all her gaiety and coquetry she never in any way appealed to me.

When watching the sun rise I am very apt to think of an old man with a heavy pack on his back. I had this idea at about seven and it still continues as an association.

I never had any fancies connected with the dawn (!)

I thought one time as I saw the sun rising through the clouds that it seemed

F., 18. I had heard a discussion about Mormonism in which a case was discussed of a husband being so cruel to seven wives that they had risen in arms and pushed him out of the house, at least that was the way I understood it. Now I thought that the sun was a person and imagined that every morning his wives pushed him out of the house and he came through the sky. (4 yrs.)

F., 17. When I was a child I had an idea that the sun and the moon were the same body and this body was not so large nor the same color at night as it was in the day time.

F., 19. I used to think that the sun was the golden chariot of God and that He used to ride from east to west in it. The rays of the sun were the lines and the clouds were the horses. (5 yrs.)

F., 19. When about five years old I thought the sun got tired staying in the house the same as I did, and that it liked to take a walk, so that was why it rose every morning.

F., 18. I fancied that the moon had been doing something wrong and the sun was coming after her. As soon as the moon saw the sun coming, she dodged out of sight.

F., 18. I fancied that the sun was a large round ball of fire. My theory was that during darkness it went to sleep and then at dawn it was time for it to get up.

F., 19. I fancied that the sun was a huge lamp which gave light to the earth. I also thought of it as a ball of fire which rolled across the sky.

F., 19. I used to think that the sun rose in the morning because the moon was shining.

F., 17. I thought that there was a big giant who pushed the sun up.

F., 18. I thought that the sun rose out of the earth when it thought it had slept long enough.

F., 18. I have always had the feeling that the sun was a benevolent sort of person who lit up China all night and then hurried up the side of the earth to bring us day.

F., 19. As a child I used to spend my summers at the seashore and used to say at sunrise the mamma sun is getting up to warm the water for the papa sun to take his bath.

F., 18. I thought that the sun was a person who went around the earth to see what people were doing. (9 yrs.)

F., 17. When a child, I looked upon the sun as a very happy man who went to bed and got up just as I did.

F., 18. When I watched the sunrise, I thought of it as a person who was having a hard time to push himself up. I used to think that when the sun rose it was a person getting out of bed and when, on a cloudy day, the sun failed to shine, I thought he was sick.

Points to be especially noted in connection with these fancies and theories of sunrise are the great richness and fecundity of the childish imagination, the constant recurrence of personification and the elaborately constructive character of many of the theories. Many of these are evidently derived from mythological stories, some from nursery tales and the forms of all are doubtless to be explained by the child's environment, but however the form may have been suggested it seems in passing through the alembic of the child's imagination to have taken on an individual character. Closely connected with the reactions of dawn, and in sharp contrast to them, are the reactions to night and darkness with its attendant fears and fancies. The specific questions under this topic were:—

Night. Cases of dread of night in advance or of darkness, with and without special fears; do children huddle and cluster? Give night fancies. Ask what darkness is; its cause.

285 out of 389, or over 73%, report night fears at some period of life and as in other cases the remaining 27% covers the cases which gave no answer to the question. Of fears specifically reported 58 are of ghosts, goblins, witches, phantoms or other supernatural beings, 42 a fear of darkness itself. Other fears reported are of bears, wild animals, indefinite animals, bogie man, Indians, man under the bed, something following or watching, eyes, toads (supposed to be a form of the devil), Zee Zees, something that will hurt, bees (probably an association from the old superstition of telling a death to the bees), and most frequent of all and generally associated with other fears is the feeling that something may seize or grab at one from out the darkness. In addition to the cases given in the return twenty-two adults, none of whom have any intellectual belief in superstitions of any kind, have stated that they still have this feeling on entering a dark room or any closed space. The character of the dread associated with night appears in the following extracts quoted from the returns.

F., 18. Several children have told me that they were afraid of ghosts in the dark and wished it would stay light.

F., 19. I do not dread the night now but I did when a child and was afraid even to go on the veranda after dark. After getting into bed I would get close to my sister. I have often noticed that it is the tendency of children to huddle together when in the dark.

F. The only case of dread of night in advance which I know is a little cousin of mine who is so much afraid of the dark that he begins to fret and worry until the lamp is lighted and the blinds closed.

F., 19. I never had any dread of night in advance but I do not like to be alone in the dark.

F., 18. A feeling of fear and horror comes over me when I go into a very dark place even if it is not night.

F., 19. When I was about twelve years old several houses near us were entered by burglars. For a long time after that I used to feel a dread of night in advance.

F., 19. I used to feel from the time I was about five until I was ten a dread of night in advance. I was always very much afraid of the dark.

F., 19. I have always felt a dread of night or darkness. I think it is mainly because I am afraid in the dark.

F., 18. I used to be afraid of the darkness preceding a storm and had a strained feeling of excitement as well.

F., 18. I have always had this fancy about darkness, that it was something material, something that would strike me if I were not careful.

F., 18. In darkness I always felt depressed, and if it were very black it was hard for me to breathe. It seemed to have a stifling effect upon me.

F., 19. When I was a small child I had a great dread of dark especially if away from home. My dread seemed to consist of a general fear combined with a fear of "somebody." At present my state of mind depends upon my general nervous condition. If excited and nervous I become fearful.

F., 18. I am afraid to go in a dark place even yet. My strength seems to leave me when I walk in a dark place.

F., 17. I imagined that at night evil spirits or fairies would come to the earth and God dropped a black cloud over the earth so that the fairies could not do as much mischief as if it were light.

F., 18. I dread night only in the country. Having always lived in a city it makes me feel lonely at night in the country.

F., 17. I have never dreaded darkness in advance and never heard of any one who did. I have not heard of any child who asked the cause of night.

F., 18. When a child I dreaded to have darkness come but I think it was because I heard so much said about the coming of the end of the world.

F., 19. I have known children to dread night in advance, when they have heard frightful stories, fearing to go to bed alone.

I know a child who imagined that darkness was an animal resembling a cow and though he was repeatedly told that it was not, was never satisfied until the doors leading from his room were closed so that the animal could not enter.

I never dreaded the night before it came but when it did come I was always very much afraid.

I never felt a dread of night in advance but I have often felt a dread of the darkness that precedes a thunder storm.

F., 34. I do not remember any feeling of dread in advance of night. In the dark a feeling of dread came over me. I do not know what I feared. I always prayed and then felt all was well.

M., 19. I do not really dread approaching darkness but I always feel as if I wished it would not come.

As darkness approached I imagined I saw shots filling the air. I saw these shots come but I was puzzled as to where they came from.

F., 18. I never feel nor have felt, to my recollection, the dread of darkness in advance, but during darkness I do not like to be alone.

F., 17. The night out of doors was always a delight to me and I had no fear, but to go into a dark room terrified me. I have overcome this feeling by will power, but I do not like a dark room even now.

F., 21. When a child and up to the age of seventeen years I felt that the worst of all things was darkness. I could not be induced to enter a dark room alone. I feared lest some evil person should seize and murder me. At present I am timid in the dark but that dreadful fear has to a great degree vanished. I have never felt a dread of darkness in advance.

F., 18. I was not afraid of night in advance, but I did fear the dark when it came. I cannot remember that I was afraid of anything but the darkness itself.

F., 18. I have often dreaded the coming of night without any special fears.

F., 19. When I have been alone on the mountains, with night approaching, I have sometimes felt a dread of the dark and a feeling of extreme loneliness and longing for companionship.

F., 16. I have a sister fourteen years old, who though not dreading the approach of night and darkness will scarcely venture into a dark room alone without a light.

F., 19. When very ill for a long time and compelled to lie in bed I dreaded the approach of night for no special reason other than it was dark and did not seem so cheerful even with a bright artificial light.

F., 19. When I was about seven years old I had a fearful dread of going blind. It seemed to me that if I did go blind it would be at night. This feeling did not leave me until I was fifteen.

F., 18. I do not remember of being afraid of the night, but as a child I was afraid of such things as tunnels. I was more afraid just when coming out into the light. I had an awful feeling that I would suddenly be pulled back by some hideous creature.

In answer to the question do children huddle and cluster: there were 220, 56% affirmatives; 43.11% negatives and 116 blanks most of which are ascribed to lack of observation; 115 report that they never had any theory as to the cause of night and darkness, and 53 no night fancies. The thirty-eight white children all made some attempt at answering the question in regard to darkness and its cause. The most characteristic of their answers are here given.

(Age 10 to 12 years.) Darkness is a place where you can not see what you are doing. Darkness is when it gets dark and you cannot see. The world turns round and then it gets dark, (8 children). Darkness is a large black cloud all over the sky, (3 children); when the sun is on the other side of the world, (3 children); when the sky is all black; something you cannot see through; a big black sheet spread over the world; dark that you cannot see; a while after the sun sets and it gets dark; the earth getting real black; when the sun goes down and makes the earth so that you cannot see, (2 children); when you see nothing but black.

Some of the theories given as reminiscences are more elaborate and contain a larger element of fancy.

F., 19. As a child I had two theories about the dark, one that it was a black cloud and the other a veil.

F., 19. From the time when I was four or five years old I imagined that night came in the form of a large man who wore a cap and gown. He seemed to fly by means of his sleeves outstretched. To this day this idea is uppermost.

F., 18. I used to think the dark was a great giant with his upper part gray and the lower part black. As he flew over the earth with outstretched arms the twilight came first and then the black part of his garments covered the sky.

F., 19. I used to think that the darkness was a great strong person who succeeded in overcoming everything but the sun.

F., 19. I imagined that the sun gave all the light and when that was swallowed by the earth, it became dark.

F., 19. I remember when about eight years old saying that the sun was mad at us and went away to leave us in darkness for awhile.

F., 18. When a child I was very anxious to know what the dark was and how it came, also who made it grow dark.

I thought that the darkness was a big black curtain which God drew over the sky.

F., 17. I have never had any fancies or offered any explanations about the making of night except to think that something was dropped over the world so that we could not see through.

I imagined that night was the moving of the clouds which went towards the west and they were so thick every evening when the sun went to guard his treasures it was forced to go behind the clouds and thus cause the darkness to prevail.

F., 18. Darkness seems a great black sheet dropped from heaven to shut out all light.

F., 20. I regarded the darkness as a monster of immense size. I thought he spilt a bottle of ink over the world to cause night.

F., 17. Darkness is a thick veil drawn over the sun so that the light is obscured.

F., 17. I fancied that night was an immense goddess with a black dress studded with stars and a moon which she spread over the sky.

F., 20. The cause of darkness was that God blew out the great lamp.

F., 18. I often thought what made it dark but supposed God did it and I was not to know.

F., 18. As a child and even now I have a feeling that something is lying in wait in the dark to spring upon me.

M., 22. When a small child I had the following theory in regard to night. I thought that somewhere there was a huge house kept by one man. In this house were a great number of wheels one-half black, the other white. When this man wanted night he gathered in the white wheels one by one and threw out the dark wheels. While he was doing this it was twilight. Midnight was when every black wheel was out and every white wheel was in. Day was produced in a similar way by the white wheels.

F., 19. When about five or six years old I always felt as though the dark were a person who was afraid of the sun and came after the sun had entirely gone away.

F., 19. About the age of nine I remember asking if the dark was n't a great black curtain let down from heaven.

F., 18. I thought that a celestial being had large wings and when he spread them out it caused darkness.

M., 18. I dreaded the night when I was a child. I had a fear of something, not ghosts or anything of that kind, but something.

F., 19. When a child I thought night was a black curtain.

F., 18. I used to think that God had a big black rag and covered up the sun and that the stars were little gold dots on it.

F., 18. When I was about nine years old I thought that a great many dark clouds were in the sky and that the moon and stars were lights in little holes—towns in the sky.

During an eclipse not long ago I felt restless and wished for it to be over.

F., 17. The only explanation I ever had for night was that the sun had gone to bed and all the curtains were pulled down while the moon and stars were small holes in the curtain.

F., 18. I have fancied darkness as something soft and soothing.

F., 18. I used to, when about eight, think the night some great monster who had conquered the day.

M., 19. I thought night came because the fuel of the big fire (the sun) had all been burned.

F., 18. As a child I thought that darkness was caused by the letting down of a spangled curtain.

F., 18. I once heard a little girl say that at night God put something big and black in front of the sun to make little girls sleep.

F., 18. I fancied that darkness was caused by the putting out of the lamp or ball of light as I called the sun.

F., 19. I used to think of night as another little girl. It was night when she slept and day when she awoke.

F., 24. When a child of seven I thought that we had light clouds in the day and dark at night and that some one had to change them. I never went so far as to think how it was done.

F., 17. I thought that the night was a big blanket which God drew over the sky and that the stars were holes. I outgrew this fancy when about seven years old.

F., 20. Night always pleased me more than any part of the day, especially when out of doors. But a dark room frightened me if I were alone.

F., 17. I have often thought of night as a dark cloud slowly descending upon the earth.

F., 18. When about four or five years old I used to think that night was caused by some one shutting a huge door in the sky and thus shutting out the light. I thought that the stars were angels' lanterns.

F., 18. I always pictured night as a beautiful woman wrapped in a long black floating robe, with a pure Madonna-like face.

F., 19. I was always very much afraid of the dark until I was about fourteen years old and whenever I went into a dark room it always seemed full of unnatural things. But it always seemed to me that these things were afraid of the sun and would go away as soon as the sun came.

F., 19. I felt that the sun was my friend and that when it came up nothing could hurt me, *i. e.*, the monster I always associated with the dark.

F., 17. If awake before light I imagined that something was in the darkness which would catch and kill me.

F., 23. Darkness to me was always something which was out of doors. I never questioned the cause.

F. I have always had a dread of being alone in the dark. Have not dreaded supernatural things but have the feeling that people might surprise me. If I am in the house with people am quite willing to go from room to room without a light but if alone in the house dislike exceedingly to go into a dark room. I have no special night fancies but if awake, I find myself listening intently for something. Darkness is to me just darkness, never asked about it, never thought of a cause for it. It always came *down* upon me.

A trained nurse reports the case of a child eleven months old which for a number of nights in succession cried as if frightened every time he awoke in the dark but ceased crying instantly as soon as the light was turned on. Three cases of night fears in adults are also reported, in which the patients stated that there was no fear of any special thing but the darkness seemed unbearable.

In contrasting the reactions of light and darkness under these two topics, it appears that a large percentage of those

answering the syllabus have, at some period of life, experienced feelings of fear and depression in connection with darkness and that the type of fancy which predominates through all its varied expressions is of a gloomy character. At the coming of dawn all these night phantoms vanish, the depression is relieved and the imagination reverts to bright, cheerful and hopeful images.

Dr. Anagnos of the Perkins Institution in South Boston furnishes the following interesting data in regard to the reactions of blind children.

"When night is closing and day beginning,¹ A. *feels* it in the air, through vibrations, in the warmer weather but especially in the early spring, whereby she tells the exact time of the morning—whether four or five o'clock—and seldom fails. She is sure that this is no thermal effect for it is irrespective of the sun's shining. It is a feeling that night has passed and a sense of freshness which tells that the day has dawned.

B. has a feeling of pleasure at dawn but is not sure that it is independent of thermal effects, as she has a stronger sensation of pleasure when the sun shines than on a dull day.

C. feels a strong desire for the dawn but thinks it is for action rather than for the coming of the light.

"Blind children share in the fears and superstitions common to seeing children, and often they have special fears, frequently of their own invention and groundless. They betray their fears as seeing children do by covering their heads. They dread the night and are inclined to huddle together and cluster; they do not want to go about alone but seek companions as seeing children do. C. thinks that stillness and loneliness may enter as largely into these childish fears as the darkness does, and this idea is borne out by the following statement of one of the pupils:—

"I do not mind going about in the dark; but I do mind being left alone,—not that I am afraid; but because it is so still and death-like. I should not want to walk about in the night."

The phenomena of dawn to a mind refreshed by sleep starts such a highly variegated wealth of imagery and seems in so many respects to symbolize the waking of the mind itself, that it seems a calamity for childhood and youth not to have had this experience, when the whole world of sight is daily recreated. Very interesting are the returns which conceive the sun as making an effort to lift itself, to get free from the horizon, disentangle itself from the trees, break loose from the sea, as angry at the clouds and pushing them away or breaking through as a victor in a contest with them or with darkness. This psychosis is best after the childish stage when the sun is completely personified as getting out of bed, pulled or pushed upward by some alien power or person, rising like a balloon or on wings, started up by a cannon, being God's eye, God himself or his lamp, a hole in the sky or a chariot with the rays as lines and the clouds as horses. It is striking to notice how

¹A., B. and C. are blind teachers.

in many of these fancies the child has no doubt immediately appropriated and adopted as its own, from floating suggestions from classical or adult conceptions, but the fact that a suggestion so subtle that it cannot be traced suffices to establish these images vitally in the mind, when so much that we teach in school with such painful elaboration is lost, shows an affinity or a pre-established harmony between the ancient adult and the modern childish mind. Just here lies perhaps the solution of the very important question whether these dim traces or germinal apperception organs should be brought out, or whether they should be refused any nutriment of myth or hint to accelerate their complete decadence in the soul to make room for more scientific ideas of cosmology. Without entering upon this question here in detail, it may be said that no doubt the law of rudimentary organs generally has its place here, which is that they should be developed at their proper stage in order to be subordinated later like a tadpole's tail. Certainly, if we are to live out our lives completely, this is necessary.

The sun to Aristotle was divine, because it had like the other planets the mysterious power of self movement. This seems most triumphant in the early hour when its movement is vertically upward against gravity. To children as to primitive man, the sun, like the moon, is a wanderer at its own free will. It floats or rolls along wherever it wishes; rises when it feels disposed to do so; and often goes now fast, now slow. There is no idea of a fixed orbit, time or rate. The whole scenery of dawn is a sprouting garden of the most variegated fancies of which no doubt the answers to our questionnaires give but a few species of the forms that really exist in such profusion. The machines that make it go or rise; the dark and light wheels that are put out and taken in alternately; the shadowy beings that hover with gray and dark vestments; in all this the soul seems to go out to nature as on few other occasions and toward few other objects. Some draw the breath with relief when the sun really gets clear of the horizon. They feel in bondage until light comes to set them free, but a touch more of fancy would give them the sun of Heraclitus, daily secreted out of the body of the world, leaving it a little colder and darker throughout its mass, a little more like a corpse. The stimulus of this as a mere sense picture to be gazed at for its beauty, sublimity, glory or joy is great. In these returns are exemplified all stages of degeneration and decay. Geography generally marks the advent of the blighting effects of science upon the imagination. The intellect has caught the sun in its net; given it a fixed treadmill path to travel so that it is no longer the Ulysses-like wanderer; appointed it to definite times; has explained the wondrous colors

and forms in prosaic terms of condensation and refraction; banished Phœbus to the kindergarten or to oblivion. We must regard these returns as the flotsam and jetsam or the fragmentary wreckage of an old world of fancy and feeling. As the polar ice cap is a relic of the glacial age that once covered half the hemisphere, so these are mere vestiges of adult systems of views which are relegated to an ever earlier childhood and made ever more ineffectual and incomplete. The question will recur, however, whether it is not possible and even desirable to keep both the poetic and the scientific standpoints in souls large enough so that each can have its own complete development without interfering with that of the other. That this is to be possible and how, is one of the functions of the new psychology, when its studies of feeling are still further advanced, to show.

Fears, that dawn will not come, in children are due first, of course, to the fact of wakefulness and loneliness with nothing of interest to occupy vacant minds. Nothing stretches time like watching its mere lapse. Again children have developed no very definite ideas of objective time. Only the adult, and he by no means accurately, carried anything like an image of, *e. g.*, that unit of time which modern chronology has fixed as an hour. Thus they have no standard to measure by save their own feelings. Again, most children have slept most of all the nights of their lives, so that for them it is impossible to have any realizing sense of how long the night really is. In sound sleep, which is unconscious, the child has almost no sense of time, and even for the adult, as modern studies show, it is hard to tell on waking how long one has slept with any approximation from subjective bases alone. The night joins on to the next morning with a very obscure and seemingly very brief interval to the healthful child. The first wakeful night, at whatever age it occurs, brings a new sense of the duration of night. This cause, of itself, quite apart from the images which often fill the dark and which are very likely painful, contributes to the "will-it-never-come" feeling about dawn. To fill time is to kill time, and *ennui* makes the desert stretches which insomnia knows so well. Especially when experiences greatly desired are ahead and barred from us by a vacuous interval which the mind abhors, there is a painful protensive experience that makes waiting under any conditions hard, sometimes almost to the point of causing irritability or even anger. The present *must* be escaped, but it lingers on, arid, desolate and interminable.

Another element here to be considered is that in darkness when the soul has in effect lost its highest sense, sight, if not well on the way toward sleep, attention is strongly and per-

haps painfully focused upon the sense of hearing, as with the blind. Again, perhaps the content of the mind consists of the spent, positive after images of the events of the recent past which are ground over to the point of tedium, or else with sleeplessness nearer morning the mental activity is more spontaneous, and instead of being reminiscent turns to the future, and reverie and day dreaming may for a time relieve the monotony. But the youthful mind cannot long work normally or healthfully when cut loose from sense and motion and left to itself. Whatever we may think of the Lockean view that all the contents of the mind come through the senses, the childish mind is very dependent on afferent impressions. When impressions are being poured into it from all sources from the external world, and when the muscles are in their natural state of tension and activity, then alone is the youthful mind normal and growing. Without these, both its work and its images soon become unreal, tenuous and falsetto, and youth instinctively seeks escape from this kind of experience and, like Ajax, it prays for light for which it has a veritable hunger. This is quite apart from the images conjured up by dreams which, faint as they are compared to waking states, are very real in the dark, and is also independent of all the reverberations in the soul of the individual of the horrors experienced by ancestral organisms in the dark, which often veiled the danger that it brought. For to the child, especially, darkness kills motion, and his very nature is activity.

What shall be said of the many children who apparently have never had these fancies? Have they really existed but been semi-unconscious and forgotten; are these the minds that are born short, obscured and clouded to the dawn, dull or less sensitive than others? The data give no hint toward an answer. Some explanatory power, however, may lie in the following consideration.

Many children are born very young in body or again in mind, and never attain full maturity or at least never pass through, however long they live, the last stages of senescence. Many facts lead us to suspect that such cases are more common in offspring of parents somewhat too young for most effective child bearing. On the other hand, perhaps offspring of those a little too old have but little or a very abbreviated infancy, childhood or youth, mature early and show early symptoms of old age. Perhaps none of us live out fully all the stages, young and old, of our lives. For some its early, and in others its later phenomena predominate. Those with few rudimentary mental organs, on this hypothesis, partially omit the childish stages, and

those whose minds abound in them repeat more fully the earlier psychic stages of life.

Again, some live mainly in the intellect, and in some sentiment predominates, and this temperamental difference may explain something here.

But a third view remains, viz.: the reactions called for or obtained in this questionnaire are probably but very few of all those evoked by the phenomena. Perhaps had they been so shaped as to evoke others, minds that gave no reactions would have been eloquent with them. But, on the whole, a child familiar with sunrise, who has never thought how it got up, what it does or where it goes at night, what it is, but has merely accepted the phenomena with no queries and with no rank growth of suggestions, certainly does seem to represent an inferior and somewhat animal stage.

Once more, we believe, such problems as are here suggested in profusion, viz.: what is involved in longing for light and dawn, its many causes, the psychic analysis of the phenomena, how it affects feeling, the typical fancies generated in the mind, etc., are all real and large, if somewhat new problems for the psychologist, and if instead of being conventional and restricted to the few hackneyed themes of the past, psychology is to be adequate to the whole experience of the race and not provincial, these must be considered. To many, these problems seem unreal; in fact, they are as real as anything in the field of mind. They challenge the investigator and cry out for explanation. Their solution is indispensable for self knowledge or for showing the source of mind as we know it.

Again, they give æsthetics a broader foundation and have a great if as yet not defined or prescribable significance for education. Spontaneous reactions of the heart, will and mind to nature in general seem to us the very best and most stimulating of all themes, richest in hints of a larger range and meaning of psychology, and full of budding new explanations of man's inner life.

The Sun's Rays. The question was: How does fancy, either in the child or adult, picture these rays either as they shoot athwart the sky or world near the horizon; are defined in floating dust; pierce crevices in dark spaces; break through clouds? Have these ever been imagined to be spears, arrows, weapons, or the sun a fighter or warrior, and if so, against what? Omens concerning being hit by rays; how are reflected beams from water, bright surfaces, etc., regarded?

The topic, sun's rays, elicited a great number and variety of fancies though the suggestions contained in the topic were largely negatived, only 54, or slightly over 11%, of those answering the syllabus having ever thought of sunbeams as spears or arrows and 176 make the definite statement that the idea had

never occurred to them. Only 32 had ever thought of the sun as a fighter or warrior. Some of the objects to which sun's rays were likened were points of a diamond, sharp and hard (F., 20, used to wonder if they would prick her), long silvery threads shooting forth from the sky, golden reeds, long strips of fire pointed at the ends, streaks of light, some thicker than others, strokes from the hand of an invisible artist, bright points shooting out from a ball of fire, fairies dancing, tiny threads holding the earth to the sun, sprays like water from a fountain, in floating dust, like a machine belt, ministering angels or patient nuns, daggers, a hay barn on fire, long, sharp needles, swords, little missionaries seeking to do good and bringing joy and happiness with them, particles of gold, knives, light from the eye of the All-seeing One, arrows bringing cheer and brightness, tiny rainbows, an electric fountain, streams of gold, each ray having a life of its own, something alive, pillars of light, things of fire, fairy soldier. Other fancies are:

F., 18. I used to think of the sun's rays as sins and close my eyes to make them appear as few as possible.

M., 16. I used to think that they really had life in them.

F., 19. When the sun's rays shoot athwart the sky, they seem to be playing tag with one another. As I watch them in floating dust they seem to be dancing about and changing color or shape.

F., 20. I think of the sun's rays as shafts or beams of light. I have never imagined these rays to be spears, arrows or weapons though I have read stories which represented them as such.

Something Tangible.

F., 18. As a child I used to feel that the rays of the sun were something like cobwebs. I remember how disappointed I was when I tried to break them as I did cobwebs.

F., 20. I thought when quite small that the bright pieces of floating dust were pieces of gold and I would attempt to capture them in my hands.

F., 18. When I was about five years old I never would go near a ray of light for I thought it was a spear and would go through me. I thought the sun was a blind man and was following me.

F., 19. A ray of sunlight illuminating particles of dust always appeared to me, when a child, as a bar, and I often tried to take hold of it and bend or break it.

F., 21. I have repeatedly watched a little boy about two and a half years old try to catch the sunbeams in his hands, groping at them and then opening his hand and looking at it in a wondering way as if he could not understand why he had not caught something.

F., 18. Often during childhood I used to try to grasp the rays of light with my hands.

F., 19. When I was small I used to try to catch the particles of dust. I thought they were drops of gold.

As a child the sun's rays as defined in floating dust had a great attraction for me. I made a great many attempts to grasp them and because I could not had a feeling of something mysterious about them. This subject especially interested me between ages of eight and ten.

F., 18. When about four to six years old I had a never ending desire to catch a sunbeam. I was told about a million times that it was

impossible but I thought it more practical to catch a sunbeam than to catch lightning as I thought Franklin did. At first I tried to catch it in a cigar box, but seeing the sunbeam on the outside of the box I thought I did not do it quick enough and that I must hide and *not let the sunbeam know what I was doing*, so I could take it by surprise. My cousin had among his treasures a box which opened and shut with a spring. One day I placed a chalk mark where the shadow always came. That night I placed the box there with a string over a beam. Next morning I went out and sat in the hay waiting for the sunbeam which at last came, but with deep disappointment I saw it *on*, not *in*, the box. But this did not discourage me. Many times after that I sat and tried to catch them, but seeing that I did not succeed, thought it due to my tools, not the impossibility. The sunbeams in the water also perplexed me. I tried to catch them for the purpose of putting them on land but after seeing how difficult this was, I abandoned it and again turned my attention to the sunbeams on land.

Sun's Rays as Something Hard or Sharp.

F., 18. When from six to ten years old I thought that the rays of light from the sun were spikes and that they protruded from the sun in all directions. I had a kind of fear of these rays, but while I was afraid to put my body in a ray of light, I delighted to hold my hand where a ray would strike it and try to look through my hand.

F., 17. I remember feeling that the sun's rays were spears with which the sun guarded itself in case of attack by an enemy. (8 yrs.)

F., 20. I always imagined the sun rays were like needles and could pass right through you.

F., 17. I have imagined the sun's rays to be spears which pierced the hearts of people who had done wrong.

F., 18. I have imagined the sun's rays to be arrows hurled against sinful people.

F., 18. The sun's rays have always seemed to me like an immense hand held up in warning.

F., 19. Instead of weapons I thought just the opposite, that the sun's rays were something to do me good.

F., 18. I thought to sit in the sun's rays made me good as I fancied they made a halo round my head. (4 or 5 yrs.)

F., 16. As a child I enjoyed sitting so that the rays of the sun fell on me and it seemed as if I was very good then.

Sun Drawing Water.

F., 18. I had heard my mother use the expression "the sun is drawing water," and my theory was that the sun must have a great many pails at the ends of these long rays and in some way when we were not looking he drew them into the sky.

F., 21. I had heard that the rays of light were formed by the sun drawing up water and I thought of them as tubes or pipes through which the sun sucked up the water much the same as we use a straw in drinking soda.

F., 19. When I saw the sun drawing water I always imagined it was taking it up in great sheets.

F., 19. I used to think that the sun was drawing water and the rays were going up from the ocean.

Reflected Beams.

F., 18. When I used to see rays of light reflected from tin pans I thought they were smiling because they were clean.

F., 19. When I was eight years old I used to think that when the sun shone on the water, the water was very cruel to send the sun back again.

F., 17. I thought of the water as being a big looking glass in which not only sun's rays but hues, etc., were reflected.

F., 18. When I saw the light reflected from a bright object I thought that the ray had been broken and taken another direction.

F., 19. As a child I used to wonder what the reflected rays were.

Sun's Rays as Connections between Sun or Heaven and Earth.

F., 18. At the age of four or five I used to think that the rays which pointed toward earth were paths to the sun, those rays which pointed upward from the sun were paths to Heaven.

F., 18. When I was four or five years old I fancied that the rays of light were sticks or rather ladders from me to the sun. I have put my hand on a ray coming through the crack and felt disappointed because the ray did not support my hand.

F., 19. When I was nine years old I heard the story of Jack and the bean stalk, and afterwards I always thought that Jack must have used the sunbeams to climb so high.

F., 17. I never thought of the sun's rays as spears or arrows, but I did fancy they were paths leading to Heaven.

F., 18. I never connected the rays of light with the sun as a part of it. I thought they were pathways of the angels and for that reason liked to stand in them.

F., 20. My fancy pictures the sun's rays as golden ladders up to Heaven, especially when the sunbeams pierce the clouds.

F., 18. I used to imagine that the rays of the sun were different strong ropes that held the sun in its place.

F., 19. When a child between four and six I was always happy to get where I could see the rays of light. I thought that they were fairy roads and at times would watch for a long while expecting to see a fairy.

Animistic Ideas.

F., 18. I used to watch the small particles of dust moving in the sun's rays and thought that they were cleaning the room, and when the sun got lower and the rays were no longer in the room, I thought that they had carried the dirt away. I used to try to cut them by putting my hand through them.

F., 18. When I was a child I pictured the sun's rays in floating dust and had an idea that the little particles were animals and I was always afraid to go near them.

M., 19. I imagined that the particles of dust floating in the beams of the sun were little insects and that they were playing games with each other.

M., 16. I used to think that the sunbeams were alive.

Fairies.

F., 18. I used to imagine that the rays of light were companies of little people each of whom had a certain place to stand. To me it seemed wrong to stand in these rays and thus to disturb the little people. But I loved to stand in the rays, nevertheless, mostly for the fun of seeing them dart back into place again. I have often put my hands in them for the same reason and would try to feel them or feel the place where they were.

M., 17. I formerly imagined that the dust in the sunbeams were fairies.

F., 18. When I was about six or seven I thought that fairies lived in the rays of the sun-light.

Personification. Physical Attributes.

" The rays of the sun seem like long slender fingers, stretching across the sky or piercing the crevices in some dark place—fingers of mighty strength—when they push the clouds aside and dart downwards toward the earth.

The rays of the sun do not seem to come with the force of a hit but just lightly touch the earth and her productions. They dance lightly and gayly from one object to another.

I used to think the rays of light were God's eyes. I had often heard that God saw every one of our actions and thought he accomplished it by using these rays of light for eyes.

F., 17. When about ten years old the rays of light seemed to be fingers reaching out for things.

As a child I pictured the rays of the sun as being long arms and these arms floated into dark spaces and gathered all the dust out of them.

F., 19. When I was a child I thought that the sun's rays were the long arms of the sun.

F., 16. I thought of the rays as the sun's arms reaching down to steal the water from streams.

F., 18. The rays of the sun, I thought, were his hands to push away the clouds.

F., 18. When a child I thought the rays of light were the fingers of the sun.

Personification. Mental Attributes.

F., 19. From the age of five to eight I remember imagining that the rays of the sun were golden threads or, at times, I thought of the rays as long lines of light. I think I was about nine when I thought how generous the sun was. I thought it was not satisfied with giving us light from the ball merely but that it sent out its rays in all directions to give more light.

F., 17. When I was about six years old, I remember asking my mother if the sun was ever selfish with its light. I thought that when everything was bright, the sun was unselfish.

F., 18. When the sun's rays broke through clouds, I felt as if they were striking through in triumph, and this feeling I afterwards found expressed in the Greek myth of Apollo's darts.

F., 17. I have imagined the sun to be a fighter contending with the gloom in miserable houses and with unhappy people.

F., 19. When I saw the sun reflected in the water, I thought that he was looking at himself and smiling. I had been told that it was vanity to look too much in the glass and I thought that the sun was vain.

F., 19. I have often thought of the sunbeams as playing with each other. I have pictured the sun's rays as brooms and as canes as they shoot across the sky. I have thought of them in floating dust as streams of water with fish in them.

F., 17. When I was a child I often used to think of the sun as the mother and of the rays as her children which she had sent out to different homes to cheer poor people.

M. C. I often attempted to catch the sunbeams thinking they were the sun's children who had come down to play with me.

F., 19. I thought of the rays of light as being part of the sun and often said, "the sun is playing peep." I liked to put my hands in the rays and see the shadows cast on the floor.

F., 18. I always thought of the rays of light as great long needles. I thought of the sun as a man having these needles and reaching out and touching the water with them.

F., 21. At the age of seven I had an idea that the sun thrust forth arrows and that it was a person trying to hit me.

F., 19. It seems as if the rays were trying to see what was going on in secret corners, as if they were inquisitive.

The sun rays always seem to me to be trying to push the clouds away and to bring cheer and brightness to the earth.

Feeling of Companionship.

I always looked upon the sun rays as jolly dancing playfellows and always felt that I had company if they were with me.

I used to imagine that a sun ray was a kind of ladder clear up to that wonderful world beyond the skies, and that if I only knew how, I could get there along the ray of light.

The rays of sunlight always made me feel as if I had company.

When I am in a room alone I get in the sunshine because I do not feel so lonely then.

F., 19. As a child I used to sit in the rays of light. It was company for me.

F., 19. I used to like to stand in the rays of the sun and especially to put my feet in them. I felt as if they were trying to warm and amuse me.

F., 19. I never had the idea that the sun shining upon me was any special good luck but I always loved to sit in the sunshine and it seemed to be a friend to me.

F., 19. As a child I always felt happier in the sunshine. It gave me a feeling of warmth and companionship.

F., 18. I liked to sit in the sun because I felt more secure.

Reflected Rays.

F., 18. I had a little theory, when about seven, about reflected rays. I thought the sun itself got into the object and then looked back at me and I used to wonder how the sun got small enough to get in.

F., 18. I used to think when I saw the sun reflected in the water or a bright surface that he was looking at himself and smiling. I thought he was vain.

F., 17. I always thought of the light on the snow as the glistening of precious gems. The sun flitting in the brook reminded me of tiny fairies dancing and flitting about. The light on a smooth surface, as a pan, reminded me of a person smiling.

F., 19. When between five and ten years old I used to think that the sun was smiling at me when I saw it reflected from a bright surface.

F., 19. When I saw the rays of light coming from a tin can or piece of glass I imagined it was smiling on some one in the sky.

F., 18. When four or five years old I used to think the reflected rays were children of the sun.

F., 17. In childhood I always thought that the reflection of light was caused by the object becoming angry and striking back.

F., 19. I always thought that the beams reflected from the water and bright surfaces were evil spirits who wished to hurt the eyes.

F., 19. I always imagined any body of water as being the sun's looking glass.

Miscellaneous.

F., 19. When as a child I sat in a darkened room and the rays of the sun were shining into the room, I have often associated them with

the bright eyes of persons peering through the iron bars of a prison door or windows. I was somewhat afraid of them.

F., 18. Another childish fancy was the rays were God's glasses and He could see all I was doing and knew when I was bad.

F., 18. When I was about eleven years old I thought of the long rays of light as similar to the sticks of an open and shut fan.

F., 20. I think I have always had a feeling of dislike for rays of light because when they appear on the floor I step on them and try to get rid of them. Although I act in this way I am fond of sunshine.

M., 28. The actions of both men and animals prove that light produces activity. I have seen many times whole herds of cattle up and feeding on a bright moonlight night. A farmer in the fall of the year will generally put his cattle in a pen when the moon is full for fear they will break out of their accustomed field and get into his harvest fields. On a dark night they lie down quietly but on moonlight nights they always seem to want to be in mischief. A canary bird from a dark corner of the room placed in the sunlight immediately begins to sing, a parrot to talk. Birds do most of their singing in the morning. Squirrels and woodchucks whistle in the morning.

OMENS

Very few omens in regard to either sun or moon were reported, only 21 out of 427 giving examples and 223 stating definitely that they had never heard any. But two of those given refer to the sun, one being expressed in the phrase "Happy the bride the sun shines on," and the other has also reference to a bride though of a different character, being a reference to the superstition, that, if during a marriage ceremony, the rays of the sun fall through the windows in such a way as to form a cross, it is a sign of death.

The omens given in regard to the moon were: seeing the moon over right or left shoulder as sign of good or bad luck; moonlight as a cause of lunacy, and moonlight on new hay or money in the hand as a sign that both will be plentiful for the coming year or month. Moonlight on the hands held over a basin of water was also suggested as a cure for warts. A number of the returns, however, stated that sunshine at the beginning of any undertaking always aroused a feeling of coming success although no definite omen was associated with the feeling.

The ideas in this group though not susceptible to classification by any hard and fast lines, since the ideas of one division overlap and run into those of another do, nevertheless, present certain type characteristics. Animism, personification of both physical and mental attributes, the feeling of companionship, sun's rays as a connection between heaven and earth, or the sun and the earth and the tangibility of the sun's rays are all clearly brought out by the returns. The idea of the sun's rays as something tangible is confined to quite young children, probably under four or five years of age. The present returns give but few data on this point, but in a collection of material derived from 84 children of the lower grades this appears to be

the age limit for this idea. In the same returns it is stated that about one-third of the children under six years of age personify the sun. Some of the feelings attributed to the sun are generosity, selfishness, sorrow, gladness, envy, anger, triumph, curiosity and vanity. Also the sun goes to bed and to sleep, plays hide and seek, takes a walk, grows tired and rests. The sunbeams play with each other and are company to children, they work and the water seems cruel when it sends back the reflected rays. All these fancies are expressed in a great variety of ways though the underlying idea remains the same.

Since Max Müller and Cox¹ traced such a vast body of myths to a solar origin, the rays of the sun are shown to have been the origin of many wonderful legendary weapons. The arrow of William Tell who, as every one now knows, was not an historical person but a solar hero, the darts of the far shooting Apollo, the arrows of Philoctetes and Ulysses, and not only many magic shafts but perhaps the swords of Theseus, Perseus and Siegfried, Arthur's Excalibur, Orlando's Durandel, the Volsungs' good blade Gram, etc., are ascribed to one common origin, viz., the rays of the sun. In an age of war they were interpreted as weapons, or more psychologically the rays of the sun, by their length and power to pierce clouds which are often personified as monsters, and gave the human mind a higher idealization of what spears and arrows could conceivably be and do in an age when the human mind was developing with such momentum that it exhausted the possibility of every phenomenon of nature to build itself larger conceptions. These rays were related to the sun gods as his chief weapon was to a great warrior. Is it not plain that childish fancies of rays like the above lend to the main contention of Cox new support not unlike that so often given to phylogeny by ontogeny? The child is only repeating the history of the Aryan race from which he sprung. What was once the terminal stage of adult development is now seen in children as but faint tentatives just timidly budding before they are nipped by contact with science, which conceives one of its chief functions to be eternal offensive and exterminating war upon superstitions. We discover here the psychic germs from which these greatly elaborated ethnic concepts grew, which poetry and art will ever fondly cherish. More yet, modern childhood is so fecund in fancy that our returns show many suggestions that never have received, but still await elaboration in adult art. Individual returns at random are—the rays are “bars between me and another world;” “ladders for fairies to dance on and go to and from heaven on;” in the water they are “the gleam of a mermaid's shimmering

¹ See his *Mythology of the Aryan Nations*, Vol. II, p. 96 *et. seq.*

gown;" "a wreath of rays around the round happy face of the sun;" "the sun's long arms reaching out to embrace and warm us;" they "pierce all crevices and pry out darkness as you pry out a stone with a lever." Many make a dust to see the particles float in a ray, blow them, or try perhaps to follow one. The rays plunge into the sea to loosen or open a way from the sun, who sinks into it as they do; they spout out on all sides like jets of a fiery fountain; isolated rays are lonesome; are often feared if it is not known at once where they enter the trees or house, and with one child this quest is a neurosis; clouds are very bold that do not fear them; the dark is many times as big and knows enough to fly at their approach. One thinks the dust separate particles of sunshine dancing; they and the rays die when the sun does; reflected rays are impertinent and rebellious; threads or ropes to hold the sun; tubes to suck up water; paths or ladders to the sky; strokes of an artist's brush, spears, daggers, with power to make good or to punish us if we are bad, full of gold dust; the sun is unselfish to send them out so freely, vain to mirror himself in the water, or cruel to break the rays at its surface, etc.

To science, all these are only rank weeds of ignorance and superstition to be extirpated, or at the best products of bad observation. Rays are only vibrations too small to be seen, following mathematically straight lines and reflected by equal angles of incidence and reflection. The atmosphere of the mind must be purged of all vapor and clouds like the moon. Here is a battle-ground, fully typical of all the rest between the imagination, which is the organ of the heart, and reason which is of the head, between faith and knowledge, religion and science. It is better to learn how fast the rays travel, how many vibrations per second, how much heat the sun emits through them, of how many candle power they are, by what prismatic action the pencil of light is fanned out into chromatic hues, for mathematics and not poetry is the origin of true enlightenment. So some would let the pedagogue loose upon all these abortions of the mythopœic faculty and make the child early ashamed of its creations. But the age of fancy comes first and is no whit less in need of full development in its nascent stage than science in its. Normally the latter develops naturally out of the former and with no opposition, each strengthening the other and each suffering if the other is undeveloped. The cultured adult, if the genius of childhood is not killed in him, reverts to all the delicious dreameries of early days and recognizes in them the seeds of whatever æsthetic appreciation he may be capable of enjoying. We should, therefore, take no less pains to develop youthful reveries in their season than mature insight in its, and above all avoid every hint of opposi-

tion which has no natural place in minds large enough for both. Every phase of this problem will receive ample psychological and pedagogical treatment when psychogenesis and education are better understood.

3. Sunset. State both sentiments and fancies on seeing the sun slowly descend in the west, when its disk first touches anything in the horizon, trees, buildings, rocks, hills. Does it enflame? As it sinks, one half and then the other, does it seem to enter the earth or water, or where does it go for the night; is it put out or absorbed?

The question: "does the sun enflame," was answered in the affirmative by 113 adults and in the negative by 78, the remaining 198 giving no answer. It is evident, however, that in one set of 210 papers the question was taken as literally meaning to set on fire, so that the negative result is not significant and the numerical count of little value though the qualitative results are interesting. The same is true of the other questions formulated under this topic, the mere tabulation being of far less interest than the diversity and quality of the sentiments and fancies expressed. As before the suggestions contained in the topic were not accepted to any great extent, the idea that the sun entered the earth appealing to only 45 while only 34 had ever thought of it as entering the water and but 31 as put out or absorbed. This, however, is by no means a negative result, and but serves to show that the mind accepts only that for which by its natural development—as nearly every paper contained some thought on the subject. 31 thought that the sun went to China for the night, 19 said that it went around the earth, 35 that it went to bed, 19 that it went behind the earth, 30 that it went behind the hills or mountains, 8 that it dropped into an abyss, 12 that it lies on the earth, 12 that it went into the woods, 4 into the clouds, 4 to its home in the clouds and one thought it went into the Sea of Galilee. The sentiments typical at sunset, given in order of their numerical importance are moral and religious aspirations, sadness, loneliness, rest, awe and reverence, quiet and thoughtfulness, peace, gladness, regret, sorrow and longing.

The topics of this group seem to fall into certain lines of division, though in many cases the characteristics of one group infringe upon those of others. The extracts from the questionnaires have accordingly been grouped to show as far as possible the prominent characteristic of each group of reactions. The question "does the sun enflame" elicited some interesting replies and it is to be regretted that the data, which was chiefly reminiscent, give little clue to the ages at which any actual belief in the power of the sun to set fire to objects was held.

Sun Enflames.

F., 18. I have often felt that there was a large fire somewhere when I saw the sun set. Everything seemed to be burning up and the sky a sea of fire.

F., 19. As the sun set in the west, I thought as a child that the big fire was almost out and needed more fuel. Another fancy was that it passed around quickly on the under side of the earth and reappeared as the moon.

F., 19. Sometimes I imagined that the setting sun was a great fire but never that it would burn anything. I regarded it as a harmless kind of fire.

F., 19. When the sunset was accompanied by brilliant lighting of the sky, I thought that the angels were building bonfires in Heaven and that they were so big and high that we saw the reflection through the ceiling of the earth (the sky).

F. I always wondered why, when the sun set, it did not set fire to the trees, because it seemed to touch them. Sometimes when the sky grew quite red, I would become quite frightened, for I thought the sun had set the sky on fire and all the world was going to burn.

F., 17. I had always during my childhood an idea that the sun was going to set the sky on fire and burn up the world. Even now a faint uneasiness comes over me when I witness an unusually brilliant sunset.

F., 19. If the rays were very red and were reflected in the sky, giving the sky a beautiful appearance, I often wondered why the trees and houses were not set on fire. At last I came to the conclusion (when I was ten) that the sun would fall to pieces if it set things on fire and then it would not be able to come up and see us the next morning. The reason the sun went down for the night was to make room for the moon and then the moon would go down so that the sun would have room to come up.

F., 17. I have often wondered why the sun did not set fire to things especially if it were very red.

F., 16. I always imagined when the sun set, it was falling from the heavens to the parts under the earth. I was always afraid it would set fire to the whole world.

F., 18. I always imagined that the sun went right down in the ground. When about 5 years old, I remember of seeing a sunset over a woods and I thought as the disk touched the trees it burned them and when it went further down it burnt the trees all up.

F., 19. I used to think of the sun as burning up the trees and houses and I was afraid the fire might spread to our house.

Personification. Mental Attributes.

F., 19. When I was a child and saw the sun set, stories of dragons and fairies at once presented themselves to me. I well remember asking my mother whether the sun had gone to guard the treasures of a king or whether it had gone to frighten away the ghosts in China.

When about seven years old it seemed to me that the sun did not go out altogether but turned its dull back to us. The sun always seemed to me to be *some one* and not a thing, even now it seems company.

F., 18. I thought that when the sun went down in the west it was tired.

F., 19. When the sun was very red at sunset, I thought that it was excited and angry.

F., 17. I thought that the sun was sorry to say good night and that when the disc just touched the top of the hill it stopped to say a last good night.

F., 18. I imagined that the sun ran back across the sky when people were asleep.

F., 18. When, as a child, I saw the sun go down behind the trees, I thought that it was hiding and that perhaps when I passed that way it would harm me.

F., 18. When the sun set earlier in winter I thought that it wanted to get into bed and be warm. In summer I thought that it wanted to stay up and not get into its hot bed. I thought that the clouds were its bed covering. (5 or 6 years.)

F., 19. It seemed as if the sun disliked to go and stayed with us as long as possible. I was always sorry when it had to go for I thought that the Chinese would not be good to it.

F., 19. Between my fourth and sixth years, I had the feeling when the sun went down that it was God who had been watching me all day going away to watch other people.

Personification. Physical Attributes.

F., 17. I used to think that the sun went into the sea to sleep. (7 yrs.)

F., 20. On seeing the sun go over the hill top, I supposed that it was putting out the light so that we could go to bed.

F., 19. When I was about nine years old, I always thought that during the day the sun tried to climb a tall tree, and when it reached the top it fell off on account of exhaustion. After it fell from the top of the tree it went behind a hill and thence back to the other side of the sky. I had this notion strongly until about eleven years old.

F., 17. As a child I thought of the sunset as the sun's going to bed and covering himself with a beautiful comfortable.

F., 18. When the sun was setting I used to think of it as a man returning from a long journey. I also thought that the beautiful red coloring was the fire from Santa Claus bake-oven and I often wondered why he baked at night when we always baked in the morning.

F., 19. When I saw the sun set over the water I always waited to see the water splash up in great sprays. When I saw the sun set on land I thought he went into the earth feet first, then body and head and finally his long arms gradually disappeared. These arms were sometimes stretched in all directions above the horizon, sometimes I thought of them as folded close to his side.

F., 18. When the sun set I had the idea that it was a very gorgeous Indian who was walking down the other side of a hill which was at the west of our home.

F., 17. I thought at one time when there was a red sunset, that it was cold down in China and that a fire was being made to warm him and we could see only the top of the blaze.

F., 20. I used to think that the sun went to bed in his house behind the hills.

F., 19. When a child from four to six I thought of the sun as a big man that had been holding his great, big lamp all day for us to see by; now he was tired and going behind the hills to his bed.

Where the Sun Goes When it Sets.

F., 18. I thought that it simply dropped off the earth and I have sometimes stopped to listen for the splash, for it seemed as if it must drop into water. This fancy still persists. I cannot get away from it.

F., 20. The sun at setting seems to me to be going out of two gates. I can almost see them close.

F., 18. As a child I thought that the sun sank into the ocean and wondered how it got lighted up again to rise the next morning.

F., 19. I thought that the sun went into the water and became the moon.

F., 19. I thought that when the sun set, it went into a valley.

M., 19. I have the feeling even yet that the sun descends in a perpendicular line behind the mountains, turns at an angle and finally reappears in the east.

F., 19. Between the ages of ten and thirteen I used to watch the sun as it sank behind the trees and I thought that the soft fleecy clouds absorbed the sun, hence their beautiful coloring.

F., 18. I always imagined that the hills in the distance swallowed the sun when it set.

F., 17. I thought that the earth opened its mouth and swallowed the sun and let it out of the other side of its mouth the next morning.

Power that Moves the Sun.

F. The sun always seemed to me a big round ball that invisible hands were pulling down behind the mountains; they pulled very slowly until the rim touched the mountain and then they gave a quick pull and it was gone.

F., 17. When a child I used to think that God pushed the sun around the heavens.

F., 18. When the sun began to sink in the west, I thought that the God who upheld it was getting tired and that it was the relaxation of his hold that caused it to sink.

F., 18. I had heard people say when the sun was setting that the woodchopper was about to cut off the limb and I wondered if there was something which held the sun up like the limb of a tree.

F., 13. The sun has the same power to rise and set that we have to walk.

M., 18. c. I thought the sun rose and set itself.

F., 19. c. I thought the sun was pulled up and down.

F. It has always seemed to me as if an irresistible force was pulling the sun down as it nears the horizon and that the nearer the sun came to the horizon, the harder it pulled. As a little child, I thought this force was God whom I pictured as a strong old man, and though I no longer think of it in this way, the feeling of something pulling the sun down remains the same.

What the Sun is.

F., 18. When the sun touched the horizon I watched it to see if it would not bounce, as it seemed so light and soft and fluffy like a rubber ball.

F., 19. When a child I used to think the sun was made of glass.

F., 19. I used to think that the ball of fire was cooling off and rolling away when I saw the sun set and disappear from view in the west.

F., 18. I have never had any particular emotions on seeing the sun descend in the west. I have thought of its being a hole in the sky and have watched it glitter.

F., 18. I used to imagine that the sun was soft and indented and absorbed by everything it touched.

M., 16. c. I thought the sun was a big fire that was put out or covered up at night.

In a collection of ideas of children three or four years old, the sun is regarded as a yellow rag, a spot of paint, a round piece of gold, a fire balloon, paper stuck up, a cake of ice, a hot orange, a big lump of gold, a ball of fire, God's eye, a heated cannon ball, a big lot of fire crackers or just a hole with the

light shining through. The idea of motion seems to enter into these earlier ideas in only a very limited degree. Ideas of its distance are also extremely crude, some children, even up to the age of seven or eight, thinking it possible to reach the sun with a long pole or that it actually touches the trees as it sinks below the horizon. Some of the reactions of those older are:—

F., 18. I never see a sunset but that I feel better for having seen it, *i. e.*, I feel that my mind and soul are greatly benefited by seeing the sun slowly descend. I have received as much good from some sunsets as from sermons.

F., 19. At one time I was at the seashore and saw the sun sink in the horizon. I was very much frightened and thought that we should never have any more sun because if it was a ball of fire it would be extinguished when it touched the water.

F., 18. When I see the sun slowly sinking and the wondrous play of colors in the west, it makes me feel as if I would like to make my life fuller, larger, nobler. It sometimes makes me feel a little sad and subdued but peaceful.

F., 19. The rays at sunset always make me think of Heaven. I have had this feeling since I was thirteen.

F., 27. When alone with nature at sunset I, at first, enjoy the stillness which prevails at that time and after that a loneliness creeps over me.

F., 19. To me sunset is the time when I have my best thoughts. I feel in a world by myself. As the sun drops out of sight the element of fancy disappears and sober thoughts and sentiments take its place.

Among the points of interest to be noted in connection with this topic is the tendency of the very young mind to accept things as they appear without any attempt at explanation. This same tendency is noticeable in those of the colored race to whom educational opportunities have come later than to the average child in the northern states, a large proportion of those between the ages of sixteen and twenty-seven, answering the question as to theory of sunrise and sunset before geography was studied, with an unqualified "I never thought about it." The tendency to personification, as in the other topics in regard to the sun, is prominent and appears in a great variety of forms, and the idea of the sun as pushed or pulled by some force outside itself is not only of frequent occurrence in children but adults also testify to the persistence of the feeling and the inability of scientific knowledge to displace it. This separation of an emotional reaction from accompanying intellectual assent is in itself an interesting problem to the psychologist, entering as it does into the whole problem of belief. In the child, apparently, the elements of belief are easily separable, their attitude towards toys and imaginative games seeming to be one in which the intellectual assent is slight and the connection easily broken. The little girl may at the same time lavish her affection on a baby constructed from a shawl, making it her companion by day and night, yet be quite undisturbed by the necessity of an

occasional laundry process and reconstruction of her baby. For this reason it is extremely difficult to discover how far the personifications of children are attended with actual belief. Precisely the same difficulty has been encountered in the study of Indian myths and beliefs. The *Wakanda*¹ of the Western tribes, accepted by the early missionaries as meaning the Great Spirit and embodying a monotheistic idea, is now proven to cover a variety of ideas, the original meaning of the word implying the power of bringing something to pass and also something mysterious, hidden or unseen. In some cases this mysterious power is evidently conceived as similar in its nature to the human will, but among some tribes the word is applied to ceremonial objects and decorations. The character of the sentiments evoked at sunset is also of interest, the predominance of those of a moral and religious nature suggesting that the establishment of the vesper service by the Catholic church was the recognition of a psychological principle, on at least its practical side.

The physical differences between day and night seem exactly reflected and expressed psycho-physiologically in the differences between the waking and the sleeping state. The waking soul is the creature of the sun and instinctively worships its creator. Without light, all things that could live at all would lack all the periodicity that day and night have brought into the world, so that all that submits to this great pulsation is heliotropic or phototropic in yet far more manifold senses and ways than are described in this paper. Most that the waking adds to the sleeping state in all higher creatures is due yet more to light than to heat, for the former is by far the greater diurnal variant, hard though it is always to separate thermal from photic effects. Sentiment no doubt long preceded science in its dim way in recognizing to what an extent we are children of the sun.

It was a bold and brilliant conception of Max Müller that the primitive Aryans, shut in their valleys by high mountains, with no political organization, little literature, science or other cares, with no alien culture to absorb, no education, and intimidated by no criticism, leading a monotonous life with herds and crops so dependent on celestial influences, weather, heat and light, should turn their attention skyward with a fresh and eager curiosity and interest nowhere else paralleled, and vent on the phenomena of dawn, evening, cloud, storm, etc., all the creativeness of the native mental vigor of their race, personifying the phenomena of nature in three thousand deities or

¹A. C. Fletcher : *Science*, n. s., 4 : 476.

¹W. J. McGee : *Rep. of Bureau of Ethnology*, '93-'94, p. 182.

heroes, giving them names which modern philology shows to be only epithets describing their sensuous effects upon minds and hearts as exquisitely attuned to them all as those of childhood, but with rare power of what Herbart deems the best culture, viz., elaborating their own ideas and working them over and over till the Hellenic mind took up the themes and wrought out a mythology and theology largely based, though unknown to them, upon the direct responses of the soul to the multiform aspects of nature, chief among which were always those of day and night, the sunrise, its progress through the sky, victory over darkness, battling with storm and cloud, and sinking to a glorious death at last. The solar heroes, as every schoolboy knows, are strongest at noon, heroes of many wars, always victorious, glorious in their own beauty and radiance, often golden haired, sometimes with one eye, exposed on the eastern hills in their infancy, slay their parents, have strange fits of gloom, and their death is triumphant transformation scene with resurrection implied. All have some, although perhaps none have all of these traits. Olaf Redbeard, Boabdil the Moor, Fethringer, Elijah in Jewish tradition, St. John of the Apocrypha, and all the great sleepers will awaken in some morning or springtide of the world. They are often strongly subject to alien and inferior powers; Achilles to Agamemnon, Apollo to Admetus, Hercules of the twelve labors to Eurystheus, Cinderella to her mother-in-law, etc., as light is at times subject to darkness. Their far-darting weapons are unerring and pierce all the monstrous dragons of darkness. Many elaborated literary products are translatable back to phrases merely descriptive of these processes, says Müller; *e. g.*, "Selene loves Endymion," means when translated etymologically, the moon loves the setting sun. "The children of Niobe were slain by the arrows of Apollo," merely states that the children of darkness are slain by the rays of the rising sun. The metaphors faded, their poetry fossilized into literal personalities, what happens every day and everywhere was given a place and perhaps a date, poetic genius developed and spiced all with details, the incidents and conflicts were raised to the moral plain and made pregnant with ethical meanings, and natural religion reached its greatest efflorescence only to be superseded by one emanating from the heart of man rather than from the heart of nature.

Before adolescence, personification of the sun has generally ceased, but the old joy of sunrise and the pathos of sunset is now felt more deeply and in a way that never fades. To many a youth its rays seem arms stretched in benediction as it sinks, and there is an echo of the old fear that it may never escape from the sea or overcome the obstacles in its pathway, and rise again, which in modern times is given new but vaguer forms,

but springs from the same psychic roots as did the old traditions. Its conquest of night; its power to put darkness to flight; to drive away old winter with all its luggage; to usher in spring and vegetation, still impresses the heart with a sense of its power as a great conquering king.

How modern poetry lives, moves, and has its being in the same realm as childish fancy, a few almost random quotations, some of which are perhaps not all with literal exactness, will show. "It drank the blood of the sun as he slaughtered sank;" "the sun is a bridegroom, the earth a bride, they court from morn to eventide;" it still shoots its burning lance, kisses the earth its bride, the pale angels of dawn that opens glorious portals and scatter the "wandering ghosts that beat the gates of heaven all night;" the night comes "fold on fold, dulling the western gold;" it "blackens bush and tree;" "draweth night's dim net;" the sun's "footprints leave the sky ruddy;" "like a withered leaf the moon is blown athwart the sky;" the sun "hails down light;" its silhouette is "etched clearly" in the eastern sky; the west is "broken into bars of color" and after sunset there is a "lifeless cloud like a dead angel lying in a shroud with lilies on her breast;" "with light and butterflies the world did seem to flicker and float as though the maker slept and in a dream imagined it;" the sun "weaves a Morris dance" of rays and shadows; "the moon rises wearily with virgin snow;" "darkness takes not sunlight in her snare;" "light and song wash night away;" it is "all a fire flaming heavenward;" "deep bosomed night with sea-scent soothes the liquid heaven;" the sun "dipped its cup" "where the eastern conduits ran red with wine;" the trees "turn old and gray" as the shadows drape them; the "sullen ledge of the clouds cast down;" the moon "swoons;" it is "a golden galleon beating athwart the azure tide" and "hurling shadows into the sea;" now it "ponders with smiling face mild and clear" or "chubby cheeked;" or toils up against the clouds in which it "founders;" at sunrise the "dazzled ether is a glister of diamond wine;" the dark is "washed" with light; Hesper rises "o'er the ashes of the sun;" the sun "beats up" the sky, or falls defeated from the "ominous dim space" and leaves the "stern blue crypt of night." Good thoughts fly to meet the morn; Helios is still a deity arrayed in gorgeous vesture driving a chariot, although the traces of its old dominion over heart and fancy are now more effaced than those of the moon.

To Aristotle the sun was a deity, which in his system must be of pure fire; so that he could not accept the theory of sun spots, which he deemed both absurd and impious. Anytus thought to correct Socrates of atheism he did not think the sun

and moon to be gods, but the latter denied the charge. To the younger Herschel the "willow leaf maculation" of the sun, which is the most minute discernible modification of its surface, was, despite the intense heat, living organisms developing light, heat and electricity, although each individual creature must be at least several hundred miles long. It has often been thought to be hell. Superstitions concerning eclipses still observed among primitive people; the general expectation of a great wind as the moon's shadow at total obscuration sweeps over the earth; the unsteadiness and great fallibility, even by astronomers, in making drawings of the corona during the precious two or three minutes, so laboriously prepared for, before the camera, which has no nerves, lent its aid; the persistent impression that the periodic sun spots influence business, crops, temperature, rainfall, or perhaps caused panics; the persistent weatherlore signs connected with solar phenomena, which Mr. Inwards has collected and tabulated,—all these we interpret as vestiges which show that as the earth, plants, animals, and the human body are in a profound sense children of the sun, the mind is hardly less so; and that as the coal, which runs machinery and warms our dwellings, is the result of solar momentum, so even our psychic activities suggest that brain function is also to some extent a vicissitude of solar energy.

For science the sun is simply a star seven hundred times as large as all its planets and one hundred and ten times the diameter of the earth, so that if the latter were at its center the orbit of the moon would be a little more than half way to its surface. It rotates on the average once in twenty-seven and one-half days. The darkest sun spot is far brighter than calcium light; its heat would melt a glacier fifty feet thick over its entire surface in about one minute; and the earth would soon vaporize if as near it as the moon is to us. It consists of twelve well established elements and as many more nearly proven. Even its hydrogen chromosphere of scarlet flame is from five to ten thousand miles thick outside of the photosphere or reversing layer, while the corona extends so far that it very likely envelops the earth itself. Observatories devoted to its study alone, like those at Potsdam and Meudon, weigh it; calculate that its energy could light and heat twenty-two hundred million worlds like ours, most of which light and heat is lost; seek to tell its story from the time when all energy was gravity; estimate the amount of attraction per century, or the number of meteors falling into it necessary to keep up its heat, calculated to be all the way from ten to fifty thousand degrees C; attempt to determine the probable consistency of its substance; while the popularizers figure out that a cannon ball at the rate of seventeen hundred feet per second would require nine years

to pass from the earth to the sun; that a train going a thousand miles a day would need two hundred and fifty-four and one-fourth years, and that the ticket would cost two and one-half million dollars.

But poets conserve their own youth and that of the race. The two belong together and it is no more possible to tell what childhood owes to their suggestion than what they owe to it. These metaphoric phrases are not elaborated or hardened into mythology much as they may owe to it. Some seem a little strained and artificial, as those of childhood rarely are, but all are outcrops of the same vein. If darkness created sleep, twilight had much to do in the creation of a large class of sentiments and fancies; and has inspired much poetry, and religion and æsthetic feeling. It is not only one of the great mood-makers, but has done much to shape the human soul and given it much, the genesis of which cannot be understood till traced back to its source. Its influence has been almost purely beneficent. The rush of the day is over and the passions and fears that haunt the night are not yet aroused. We are in the tranquillity of the golden mean with nerve tension relaxed, all the senses at rest, and motion, effort and work suspended. Its mild melancholy seems of the benign and wholesome kind and perhaps tends to make the soul immune from the acute forms of depression. Its sweet disphoria is the counterpart of the euphoria normal when day breaks upon rested spirits with its wondrous transformation scenes. Civilization has developed evening, the sad end of the day, or artificially prolonged twilight, which has brought added duties to the jaded soul, brought the sadness of study and wisdom and robbed us of the optimistic, upward striving hour of dawn.

Twilight fancies. What sentiments are typical with you when alone with nature between sunset and dark; is there distinct depression, rest, reverie; of what kinds? Does the mind awake as sense rests; are such experiences loved or shunned? Poetic expressions.

Very closely connected with the feelings and sentiments typical at sunset are those of twilight. It is the hour of rest, reverie and introspection, of bodily and mental relaxation. Sentiments given as typical are reverie (152), rest (127), poetic feelings (104), moral and religious aspirations (48), depression (50), regret and sadness (44), peace (27), loneliness (32), happiness (21), awe and solemnity (17), fear (6), restlessness (6), reverence (3), ambition (2), restful sadness (10), and tenderness (2). The twilight hour was loved in 273 cases and shunned in 79 while 32 had no special preference in regard to it. The question, does the mind awake as sense rests, though directly answered by only 114, is very fully brought out in the returns, the character of the mind's activity being well illus-

trated. On this topic there is a difference between returns from the white and colored races. The pleasure taken in twilight by the former is in all cases of an intellectual or emotional character while in the latter dusk is liked because it is cool, pleasant, study or work is over, etc., while the question, What do you think about in the dusk? was answered in the majority of cases by "nothing special," "do not have any special thoughts," "think about supper time" and "bed time." There is also dislike of being alone in contrast to a preference for it on the part of the whites answering the question. 80% of the colored returns expressed this feeling, which was rare in the other returns. It would, however, be unfair to infer complete absence of poetic feeling inspired by sunset and twilight in the colored race, as the apparent lack may be due in part, at least, to limited power of expression rather than the absence of the feeling itself.

Of 312 whites 104 stated that they either felt like writing poetry or recalled poems or poetic expressions which had been read, 57 that they did neither, while 151 gave no direct answer to the question. A part of this last group, however, gave evidence of poetic feeling in their attitude toward nature and sentiments expressed in regard to sunset, twilight and dawn. 44 expressed a desire for music at this time, usually for that of a subdued, dreamy character. Evening hymns were a favorite method of expression and in instrumental music the preference was for that which expressed the sentiments typical for the twilight hour. A few gave lists of their favorite hymns and poems in connection with twilight. Those occurring most frequently in the lists were: Lead Kindly Light, Now the Day is Over, Ave Maria, Longfellow's Twilight, and The Day is Done, The Lost Chord, Crossing the Bar, Twilight is Stealing over the Sea, and Gray's Elegy. The similarity of the lists is doubtless due to the fact that most of the poems mentioned had probably become familiar in the school curriculum. The sentiments typical at twilight are fully illustrated in the following extracts.

Moral and Religious Sentiments.

F., 20. When alone with nature between sunset and dark all that is good and holy in me seems to stand out very clearly.

F., 20. When alone with nature between twilight and dark I always feel nearer God. There seems to be a hush over the earth as if it were receiving an evening benediction.

F., 19. God and heaven seem nearer to me then than at any other time.

F., 20. I think that I will be a more dutiful daughter and will always try to be to my brother all that an older and only sister should be.

F., 19. My thoughts at twilight turn to the Creator of beautiful nature, and a calm and quiet feeling takes possession of me. A distinct feeling of rest and peace comes over me.

F., 23. I feel the goodness, greatness and power of God, and I also feel as if I wished my life to be better and nobler.

F., 18. When alone in the twilight I feel a great love for all things, I have a feeling of satisfaction with myself and the world at large. My mind becomes more actual, in that I can imagine constructively with greater vividness than at other times.

F., 18. When alone in the twilight, from a child, I always had a desire to be better. I never felt like laughing or talking. I always felt the solemn feeling pass away when the lights came.

F., 17. Twilight is apt to make me think seriously about right and wrong.

F., 20. I feel more than at any other time a divine or higher power something which fills me with awe and satisfaction.

F., 17. I think such experiences make me better. They seem to bring up my best feelings.

F. I always long to do something good, something for the cause of humanity. My love goes out to everybody and everything at this time. I have had this feeling since I was a child.

F., 18. I think that at twilight there is a tendency to moralize in a dreamy way.

F., 25. When alone with nature at twilight I often have a feeling of peace and solemnity with a strong sense of protection and a love for all nature. Sometimes I have a realization of a grand, noble purpose for everything and long to do something.

F., 19. When alone in the twilight an overwhelming sense of all that is good comes over me. I like to be alone at that time.

F., 18. I always feel ambitious and as though I ought to be better and do some good in the world.

F., 18. I always have a desire for music at twilight and try to compose something myself, not to play the old things. As a child, my twilight mood was disagreeable to me, now it is the best mood of the day.

F., 17. I like to listen to music that is soft and low.

F., 18. Twilight seems to be a time when we can tell our most secret thoughts.

F., 19. My greatest desire is to sing, to sing something soft, low and sentimental. The slightest noise disturbs me and makes me irritable. To me this is the best time of the whole day.

F., 20. When alone with nature between sunset and dark a feeling of reverie steals over me. Rather a peaceful feeling takes possession of me. My mind seems very active and my thoughts are usually pleasant.

F., 19. I do not feel depressed between sunset and dark, on the contrary, I feel happy and rested although solemn.

F., 18. When alone with nature in the twilight my habitual feelings are those of depression but though I feel depressed I rather like the mood.

F., 18. When alone at twilight I like to play the piano or sit and dream.

F., 20. If I have been depressed during the day the feeling is increased by twilight, but if, on the other hand, the experiences of the day have been pleasant ones I like to dream about them.

F., 17. At this time of day I feel rested, refreshed, and very light hearted and gay. I enjoy telling comical stories and jokes and this is the time when I usually tell most of them.

F., 19. I never feel depressed at twilight. I rather welcome it. It was then I allowed my imagination to work. Sometime my poetic feelings would be aroused and I would muse over various beautiful thoughts and sayings about nature which I had either heard or read.

F., 16. When I have to study in the evening, I do not indulge in my twilight mood because it renders me physically unfit for my work. I dream and dream when I am playing and see and hear the most beautiful shapes and sounds. The desire to put all the sounds I hear into music that I can play is so great that if I do not succeed I am exhausted for the rest of the evening.

F., 18. In spite of the fact that I enjoy the feeling of rest and reverie at twilight I know it is anything but good for me. My will power is not strong enough to keep my mind from reverting to those things, if I have spent much time with them, long after twilight is past and study hour begun.

F., 17. At twilight I always have a feeling of rest and reverie. I like to sit perfectly quiet and think about nothing special but just let my mind wander from one thing to another. At this time of day I often sit at the piano and play a dreamy piece of music very softly. I am very fond of the twilight mood.

F., 18. When I was very young there was no difference between twilight and daytime for me. The only way I ever noticed it was that I would go into the house instead of out doors to play. Later, when about ten years old, I became very poetic in twilight, so much so that I once composed a poem, very bad poetry of course. At present I day dream and plan during the twilight. I love it.

F., 19. When alone with nature at twilight I have a feeling of restfulness and reverie. I can not remember this feeling as a child. It has only impressed itself upon me since the age of 13.

F., 18. In twilight my mood is one of rest and reverie. I like this twilight mood. It is then that I feel many things that I cannot put into words. I often long to know some beautiful piece of poetry which would express my feelings. Then, too, I wish to sing something strangely wild and beautiful. I desire something weird and sad never anything like the common fast pieces of to-day. I am disappointed for I can do neither of these things.

Miscellaneous.

F., 18. I never had any twilight fancies.

F., 19. It always seemed to me that twilight was the time for all sorts of ghosts and goblins to be prowling around and I have often fancied that I saw them and would be afraid.

F., 18. Twilight stimulates me to physical activity. My spirits are highest at that time.

F., 21. The only feelings that I ever had at twilight are those of loneliness and fear.

F., 17. It is a time when you fancy you see a great many things.

F., 21. c. Yes I want the lamp lighted because I am afraid of the ghost.

Mind Active as Sense Rests.

F., 18. I do have a feeling that my mind becomes more active at this time for I feel more like studying my lessons and can remember them better than at other times.

F., 20. When I am alone at twilight I have a feeling of intense longing, for what I do not know. I have had this feeling since I was about twelve. My mind does not become more active. I am inclined to wander about on many subjects. At twilight I love to talk of things which seem impossible to talk about in broad daylight.

F., 19. When alone with nature at twilight a decided feeling of restfulness comes over me. My mind seems to awake as sense rests

and the feeling is so delightful to me that I am reluctant to have the lights lighted.

F., 17. When the light goes away I feel I can devote this time to day dreaming and my mind becomes more active along this line. It seems as if it would not work along other lines then.

F., 18. My mind is duller at twilight than at any other time.

F., 19. I always have a dreamy feeling and never feel like doing exact thinking. I feel a lessening of my mental powers at that time.

F., 19. In the twilight my mind is quite active in one sense. I usually dream over many things and that is the time I pick myself to pieces.

F., 19. When I am alone in the twilight I have a feeling of utter loneliness. My mind becomes stagnant. I feel as if I could never concentrate my thoughts.

F., 19. As the light goes away I can dream better but not think better about lessons.

F., 19. At this time my mind becomes sluggish.

Twilight Loved or Shunned.

F., 19. At twilight I usually have a depressed mood if I am compelled to remain inactive. If I can take a walk, play the piano or do something else, I can rid myself of this depressed feeling to some extent. I often wish there were no twilight as my twilight mood is very disagreeable to me.

F., 17. Between the ages of four and twelve I always had a feeling of fear when alone with nature in the twilight. It seemed when a tree was dark it was a goblin and would harm me or when the leaves rustled in the breeze that some one was running after me and would catch me. Now I enjoy the twilight.

F., 18. I like very much to be alone with nature between sunset and dark. There seems to be rest at that time. The beauties of nature pass through my mind, I love to be out at this time and during the summer always look forward to going to a pond near my home to watch the sunset.

F., 19. As a child I think I did not like twilight. At this period of the day I was restless and my mind seemed more active. The reverse is the case now.

F., 19. I always love such experiences, never shun them. I enjoy this part of the day more than I do any other part of it. I often think of poetry and poetic expressions.

M., 19. As a child I was always restless during the twilight and longed for the lamp to be lit. Now I usually have a feeling of restfulness and a mood of reflection. I enjoy the twilight though not as much as sunrise and sunset.

M., 18. When a child the twilight gave me an uncomfortable feeling. Now I enjoy the twilight.

F., 20. I never liked and do not like now the twilight hour. A feeling of depression and foreboding comes over me but as soon as the lights are on the burden is lifted.

F., 18. When I was a little child I did not like the twilight and used to cry for a light.

M., 18. When a child I used to be afraid of the twilight. I wanted to get inside a house and stay there. I was afraid of animals especially dogs. I was afraid a dog would bite me. At the present time I hate twilight, it makes me feel so sleepy. It does not stimulate me to poetic expression.

F., 19. I have never liked twilight. As far back as I can remember I always wished the lights to be lit as soon as it began to get dark. I

never like to be alone with nature in the twilight. I never felt that I could see better in the dusk nor was I moved to poetic expression.

F., 17. If I was left alone in the twilight when a child I was afraid. I imagined I saw things stealing stealthily toward me. Now it is my most pleasant time. It never stimulated my poetic sense because I have n't any.

F., 19. When I was a child, from about the age of five till twelve, I loved the twilight and looked forward to it every night because my sister would play for us and my brother and I would sing till the lights were lit.

F., 19. I love the twilight now but when I was a child I did not like it. From about six to thirteen I disliked it.

I love to sit in the twilight and listen to music or play the piano.

F., 18. Between sunset and dark I usually feel sad, sometimes I dream of the future. I have no distinct depression. I do not think that the mind awakes as sense rests. I shun such experiences.

F., 18. When from four to six years old I never would be alone in the twilight but now I love to dream in the twilight.

F., 17. When a child I used to like to go off and sit on a log and think of all the beautiful things about me and about what I would do when I grew up.

F., 18. When a child I did not like the twilight. It seemed to make me discontented. At my present age I rather like the twilight.

F., 17. To me twilight is the best hour of the day and I long for its coming.

F., 17. When a child, up to age of thirteen, I did not like the twilight now I love the twilight.

F., 18. I love to sit in the twilight alone and listen to music or play the piano.

F., 19. I love the twilight out of doors but when in the house do not like it at all.

F., 18. I never used to love the twilight but I do now.

The reactions of the blind are contained in the following notes from Dr. Anagnos.

"With the children twilight is welcomed as the beginning of a period of rest after a busy day.

All three teachers find the twilight hour, when alone with nature, pleasurable and conducive to reverie; they feel no depression but, rather, a sense of awe. C. thinks the mind does not awake as the body rests; A. says there is no relaxation and that the experience is not peculiarly restful. B. finds it restful.

"A. thinks that pleasure in descriptions of sunrise and sunset depends upon one's appreciation of beauty of diction and upon one's innate love of the beautiful and delight in being in it or surrounded by it. 'The best of sight is in the brain.'"

In connection with these returns, the influence of twilight as a factor in moral and æsthetic development becomes an interesting topic. How far are these moods of introspection and reverie normal and healthful? Are they good for all or are there temperamental differences which make it desirable to exclude in one case what is distinctly developmental in another. There is undoubtedly a lowering of tension in the mental as

well as in the physical faculties and the sentiments and fancies are attuned to a minor key. That this mood, even when tinged with sadness, is enjoyable is certified in many instances and in the minds of most of the writers there seems little doubt as to its beneficial effects morally. A few, however, recognize that there may be danger as well as charm in a too frequent indulgence in a mental state in which vigorous thinking and the supremacy of the will are in abeyance. It is a mood of mental and physical relaxation, the hour which invites confidences and emotional states are in ascendancy. To the homesick girl twilight is a time to be shunned. There is, too, in some of the papers, a note of fear and depression as if the shadow of coming darkness producing a lowering of the mental tone. This is especially noticeable in children and in the returns from colored students, and is not usually associated with any particular fears or superstitions. Does it point backward to an ancestral origin? The marked predominance of moral and religious aspirations in the sentiments given as typical for twilight seems to indicate that the influence of the twilight reverie may be an important one in moral development but the data do not clearly show whether these emotional states are those which later enter into the practical life and find their expression in action or whether they partake of the character of the small boy's reflections, who asserted that he had the blues every night on account of his badness during the day, but always slept them off before the next morning. Noble emotions nobly expressed in action mean moral growth but a dreamy moralizing, which finds no issue in voluntary activity, tends rather to an obliteration of the moral consciousness and to a lessening of the capacity for vigorous mental effort.

In reviewing the fancies, theories, sentiments and feelings of childhood toward the dawn and darkness, sunrise, sun's rays, sunset and twilight we find running through them certain lines of classification. The ideas themselves may be derived from many sources, many of them are of mythologic origin, some have their seat in folk tales and nursery lore, while others, though hardly to be classed as scientific, are evidently resultant from early introduction to science; some can be traced to no origin found in immediate environment. But whatever the source, the child's mind accepts only what appeals to it in that particular stage of development in which it happens to be. Mere presentation of a suggestion does not insure its acceptance, while that which is in harmony with the mental activities is readily assimilated and undergoes in the child's mind whatever transformation may be needed to suit it to his own special purposes. The constructive character of many of these sun fancies is of especial interest. In all the attempts which have been made to force

modern ideas of civilization and religion upon primitive peoples, has not precisely this same psychological fact been the stumbling block in the path of the missionary and would be teacher? Does not the study of the child's mind furnish us with pregnant suggestions for the study of the primitive mind? A comparison of the ideas set forth in the present returns with some of the ideas common to the myths of the North American Indian tribes, many of whose myths and traditions are of solar origin, presents some points which are of interest. Summarizing as briefly as possible, the ideas contained in the various topics, we find that under all the varied theories of sunrise lie two fundamental ideas: it is pushed or pulled up by some power outside itself, or it moves by its own power. The external power may be God, the clouds, light, heat or some unknown force, the power within may be of any sort, from an imaginary clock work to a complete personification including both physical and mental attributes. Dawn and darkness may be personified as themselves entities if they may be regarded as attributes of the sun. Sun rays may be children of the sun or they may be his fingers, arms or eyes, they may push away the clouds or reach down to steal the water from streams and ponds. The sun is selfish or generous, angry or pleased, sorrowful or smiling, vain, envious or triumphant. His rays may be imagined as something tangible or paths leading from earth to heaven, or they may be regarded as having life in them and bringing a feeling of companionship. Love of light and dread of darkness are characteristic. Things joyous, bright, beautiful and hopeful are associated with the beneficent powers of dawn, thoughts of dread, depression, gloom and a general lowering of the activities belong to night and darkness.

In classifying Indian myths Major Powell¹ distinguishes four stages in the growth of mythic philosophy. To the first of these he gives the name of hecastotheism, the stage in which supernatural powers are attributed to both animate and inanimate objects, an all pervading animism which answered the questions of how and why to the savage mind. In the second stage or zoötheism this attribution of extra-natural and mysterious potencies is confined to animate forms and animals, usually by reason of some special quality, as strength, swiftness, cunning, etc., become deified. In the third stage, to which he gives the name physitheism, the agencies of nature, sun, moon, stars, rain and wind become personified and exalted into omnipotence. The fourth stage, which includes the domain of the spiritual concept, has not yet been reached by any of the Amerindian tribes. None of these stages exist in pure form

¹ Powell: An. Rep. of Bureau of Ethnology, 1879-'80, p. 29.

but overlap and exist together in the various stages of development. The second of these divisions, there is nothing in the present subject to draw out, but the animistic and physitheistic stages are clearly traceable in the development of the child's imagination. Turning first to the Indians' attitude toward the dawn as expressed in myth and tradition, we find among the Navahos¹ many traditions of wonderful houses built by the gods and by the mystic founders of their race. A keen appreciation of color and the beautiful effects of morning light are manifest in these stories. These mythic houses were built of pearly shell and turquoise, of the mists of the dawn and the brilliant colors of sunset; they were covered with woven rainbows, gorgeously tinted clouds and all the richest hues of earth and sky. The ceremonials still used in the house building of the Navahos have reference to these mythic abodes. The door is still placed toward the east so as to be directly open to the beneficent influence of the dawn god, who was the benevolent nature god of the south and east, while the sunset god was not always so benignant. ²Among the Sioux, in illness, prayers are addressed to the dawn god for health and life. Among the Navaho mountain chants given by Matthews³, the dawn is described as the Daylight Boy or the Daylight Girl and the curtain of daybreak as hanging in beauty from the land of day or the land of yellow light. The feeling in this is not far removed from that in the Vedic hymn to the dawn where "She rose up spreading far and wide and moving toward every one. She grew in brightness wearing her brilliant garment. . . . She shone gold-colored, lovely to behold," and the prayer addressed to her is "Shine for us with thy best rays, thou bright dawn, thou who lengthenest our life, thou the love of all, who givest us food, who givest us wealth."

Among the Navaho myths is one of the origin of the sun and the manner of its first rising which wrought out with much detail and wealth of imagination. ⁴When the Navaho first ascended into this world they found only darkness. They prayed for light but their prayers availed nothing. So they sent for the turquoise woman and the white shell woman who lived in the Ute mountain. These two women told the people to have patience and their prayers should be answered. Now night had a familiar and this person whispered in his ear, "Send for the youth at the great falls," so night sent a shooting star as a messenger. The youth came and said to the turquoise woman "you should know what to do," so with a crystal dipped in

¹ Mindeleff: An. Rep. of Bureau of Ethnology, '95-'96, p. 487.

² Dorsey: An. Rep. of Bureau of Ethnology, '89-'90, p. 468.

³ Matthews: An. Rep. of Bureau of Ethnology, '83-'84, p. 463.

⁴ Stevenson: An. Rep. of Bureau of Ethnology, '86-'87, p. 276.

pollen she marked eyes and mouth on the turquoise and white shell beads and forming a circle around these with the crystal she produced light, but it was insufficient. Then the twelve men who lived at each of the four cardinal points were sent for. In their presence the turquoise woman sang a song but even that failed to bring the light. Then the twelve men of the east placed twelve turquoises at the east, the twelve men of the south, twelve white shell beads towards the south. And in like manner twelve turquoises and twelve white shell beads were placed toward the west and north and with a crystal dipped in corn pollen encircled the whole, but still the light did not come. Then the turquoise woman held the crystal over the turquoise and it blazed and "the great light" grew exceedingly hot. The men from the cardinal points tried to raise it but in vain. Finally a man and a woman appeared, from whence they knew not, who offered to raise the great light. But they succeeded in raising it only a little way. Then they made four poles, two of turquoise and two of white shell beads and with these the twelve men from the cardinal points raised it. But still they could not get it high enough to prevent it from burning the grass and the people. Then said the people, "let us stretch the world;" so the twelve men stretched the world and the sun continued to rise, but still it was not enough and the people crowded everywhere to find shade. Then darkness commanded them to keep stretching and at last the sun was just right. Then the turquoise woman commanded the twelve men of the four cardinal points to go to the four points of the compass to hold up the heavens and this they are still doing to this day. In this single myth, though abbreviated and stripped of many of its details, we find many of the elements which have appeared in the sun fancies of children, animism, personification of darkness, a constructive theory of sunrise and in its color imagery an appreciation of æsthetic effects of sunrise.

Personifications of the sun are almost universal among the Amerindian races. In Sia myth¹ the sun is personified as a warrior. He wears fringed and embroidered garments of deerskin and carries a bow and arrows; he wears a mask with eagle plumes to protect him from the view of the people of earth. He stops for breakfast, dinner and supper in his daily journey from east to west. The Utes² believe that once the sun roamed over the earth at will but having scorched the earth by coming too near, he had a fierce conflict with Ta-wats, the hare-god, and having been conquered was condemned by a council of the gods to ever after cross the heavens in an ap-

¹Stevenson: An. Rep. of Bureau of Ethnology, '89-'90, p. 36.

²Powell: An. Rep. of Bureau of Ethnology, '79-'80, p. 24.

pointed way. In indian myths the sun is usually masculine and the moon feminine, but sometimes the reverse is the case. In Cherokee myth the sun is a mother who every day stopped in the middle of her journey across the sky to dine with her daughter. She hated the people of the earth because they screwed up their faces whenever they looked at and was jealous of her brother, the moon, because he liked the earth people. Because of this, every day as she came near her daughter's house she shot down such sultry rays that the people died by hundreds. So they sent the rattlesnake to kill the sun but meeting the daughter of the sun coming from the door he killed her instead. Then the sun grieved and wept so much that the earth was darkened. So the earth people sent a number of their young men to the "darkening land" in the west to try to bring back her daughter, but having failed in the attempt the sun gave herself up more completely to her grief. At last, the sun dance was devised in the hope that it might divert her, and becoming interested, she at last smiled again. Among the Eskimo¹ the sun and moon are sister and brother and the sun is ever pursuing the moon, and when she comes near the moon dodges behind the dark to escape observation. In Sia² myth, the sun was created by two mysterious old women who having finished it dropped it behind a high mountain whence it rose of itself. The moon was afterwards created as companion and brother to the sun. Among the Hidatsa³ the worship of the sun is animistic. They have no idea what it really is, but sacrifice to it as a mysterious power. The moon is the sun of night.

The sun's rays as connecting paths between earth and heaven is a frequent idea in primitive myths.⁴ In Cherokee myth, the earth is fastened to the sky with cords but "no remembers who did this." The children of the sun travel on the path made by the rainbow and sunbeams to reach the home of their father. Among the Sia the war heroes cross the land of the cloud people on the rainbow bridge as did the heroes of the Norsemen.⁵ Among the Eskimo, the aurora forms this bridge and its light is due to torches held in the hands of spirits to guide souls in their journey to the spirit land. In Navaho myth,⁶ rainbows and sunbeams were regarded as consisting of "layers or films of material, and were carried about like a bundle of blankets."

¹Turner: An. Rep. of Bureau of Ethnology, '89-'90, p. 483.

²Stevenson: An. Rep. of Bureau of Ethnology, '89-'90, p. 35.

³Dorsey: An. Rep. of Bureau of Ethnology, '89-'90, p. 513.

⁴Mooney: An. Rep. of Bureau of Ethnology, '97-'98, p. 239.

⁵Stevenson: An. Rep. of Bureau of Ethnology, '89-'90, p. 39.

⁶Mindeleff: An. Rep. of Bureau of Ethnology, '95-'96, p. 488.

In comparing these sun fancies of a primitive race with those of civilized children, one can but be impressed with a similarity in the lines of mental development in the ontogenetic and phylogenetic series. The literal acceptance of things as they appear comes earliest, then as questions of how and why arise, the constructive imagination deals with the materials at hand, moulding them into fantastic shapes and varying forms, yet beneath all the diversity and variety run certain types of ideas in which we can discern a uniformity in the modes of mental reaction to objects of nature. Are not these childish and fantastic interpretations of natural phenomena a normal stage of growth in the mind of the child, a sort of mental gymnastics which play their part in development of the creative powers of the mind? Have not those who have never watched the sun rise and set missed something of real educational value, apart from the æsthetic enjoyment? Is not the precept¹ of the Ottawa Indians expressed in their third commandment worthy to be followed?

"Look up into the skies often, by day and by night, and see the sun, moon and stars, which shineth in the firmament, and think that the great spirit is looking upon thee continually."

One characteristic psychic change of adolescence is seen in its reactions to sunset and twilight. Little children show clearly the physiological effects of the withdrawal of the immense stimulus of light and grow quiet, perhaps a little depressed, or, if over tired, uneasy or sleepy. They dislike solitude, are more or less definitely timorous, cling to others, etc. It is the chief story hour. Sunset is beautiful for its brilliant colors and other optical effects, and their fancies about it are chiefly physico-mechanical or anthropomorphic. With the pubescent all this is changed. The hour of closing day, when the soul is directly exposed to its influences, opens up a new life of sentiment and mysticism. As sense is dimmed, soul comes forth. There is a deeper, sacred, symbolic meaning to it all. Conscience awakes, if not in the form of reproach, in aspirations for a new and better life. The peace and purity of the evening sky is reflected in the moral nature. The isolation of gathering twilight brings solitude; the soul is alone with itself face to face with duty and ideals. There are new longings for a larger, higher life, a desire for more self-knowledge and self-expression. A sunset is a sermon, and "betwixt the gloaming and the mirk" is the time for music, favorite hymns, for

¹ M. E. Chamberlain: Precepts of the Ottawa Indians, *Am. Jour. of Folk-Lore*, Vol. V, p. 332. These are the precepts by which the Ottawas were governed in their primitive state before coming in contact with the white races.

heaven and God seem near, as well as for philosophic thought and reflection. It is the hour, too, for reviewing the day, for moralizing, dreamily though it be, and for resolutions for the future, for ambitious plans for adulthood, castle building and reverie, often the best expression in the young of spontaneous psychic growth, when every one's muse, if he has one, stands nearest him. In the great hush and peace, the imagination is kindled. Much is thought and talked of that would be impossible by garish day. It is for plain, lucid prose, but now is the time for reading or even writing poetry, when so many in our returns invoke their muse. Some would compose new music; sing something wildly weird and sad; tell sweet thoughts, if only to themselves or an imaginary companion; let the mind wander away and away; shunning every noise and intrusion in an abandon of delicious depression, which is perhaps an after effect of the crude childish fears which now in large measure and rather suddenly fade. Pedagogy, especially that of religion and art, have here a great opportunity and perhaps will one day rise to its duty and construct a vesper service that, while not without shelter and comfort exposing the soul to all the sensuous phenomena of slowly gathering night, will devise adequate expression for the instincts that now turn the soul inward; make it feel the need of protection and trust; preform it to walk by faith and not by sight; strengthen the feeling of dependence; anticipate the evening of life and the great sleep that wakes not. This is the way the soul should descend into the dark valley, "like one who wraps the drapery of his couch about him and lies down to pleasant dreams." This is the hour of sentiment and should be sacred to its culture.

This questionnaire does not follow declining day to the blackness of night. As darkness becomes complete, the waking child feels its helplessness. It can neither resist or fly, and they realize that any creature that had eyes that could pierce it would have it at its mercy. Vestiges of the throngs of night terrors of which we perhaps inherit at least the predispositions may be revived and pass beyond control and shake the very soul. How phantasmal these are is seen in the fact that many suffer from this a lifetime and never tell and never take active precautions to fence out even the most familiar phobia objects.¹ Something seems moving, noises are magnified, flitting forms fancied, dim outlines take horrid shapes, unseen hands are just ready to grab, we must look behind, we do not fear ghosts but our muscles are tense and we are all goose flesh, the head is covered at night almost to suffocation, every crevice of the bed-

¹See *A Study of Fears*, by G. Stanley Hall, *Am. Jour. of Psy.*, Vol. VIII, p. 183.

room is examined and the light is falteringly put out with a prayer, the breath is held, often very vivid and definite pictures are painted and may persist and recur nightly for years, etc. Sight gives us warning of approaching touches, but in the dark they may come with a shock, and that human nature and nerves must abhor. Darkness first divorces thought from sense, which it is prone to follow so closely in childhood, and as fear and pain were perhaps in the unhappy past stronger stimuli than pleasure and so now are most prone to arise. The old night of ignorance, mother of fears, yet rules our nerves, and its images are still "*freisteigende*." From many points of view, hygiene, morals, æsthetics, the pedagogic importance of the early stages of acquaintance with darkness is great.

Man's control of fire enables him to defer night. Evening is the result of a progressive evolution that has taken man farther and farther from the life of nature and made him already nearly semi-nocturnal. Primitive races linger about the camp fires a while and recount the past, converse and still the mind for repose, and are up with the dawn. Many birds and animals retire with but are up before the sun, prolonging day at its best and not its worst end. This is explicable by the laws of fatigue that tired brains need greater stimulus to keep them awake than rested ones. With civilization even curfew is abandoned, hours are later, lights brighter, excitement greater, and more artificial, and as the fatigue sense is itself fatigued abandon grows, reserve forces are drawn on and neurasthenic states invited, abnormal excitants made habitual and the normal ones of morning neglected. Childhood still tends to follow the sun in its periodicity of activity and repose, but when after twilight has begun its slow somnolent magic, its charm is suddenly broken and the lights are turned on, fatigue is defied, and a fevered state of mind that is marked by abandon. Abandon supervenes as the hundreds of thousands retinal fibres carry inward the sudden arousing effects of light that are over-exciting and to which the system is slowly broken in to with such adjustment as it can make. City life is especially hard on the normal influences of night and many a cure is wrought by re-establishing its power in the country.

One cause of the noise, frolic, wildness, rise of spirits, talkativeness, excitement, etc., when the lights are brought in, is perhaps that twilight had already begun its recuperative effect and accumulated a small stock of fresh energy, which according to some theories, because but just accumulated, was more labile and not yet fully diverted to anabolic processes. This view is favored by some mentions that it lasts half an hour or an hour before fatigue recurs. Another factor is that the artificial light comes suddenly and not slowly as daylight fades and so has

some of the effect of a shock, discharging large stores of energy. Again evening illumination is concentrated in jets, flame, etc., which focus attention instead of being diffused like daylight, and finally its physical quality, color and composition is different. Snatched backward against the currents that tended to sleep, some flush with the slight degree of awakening become tense and alert, from talking, which the bashful can do best in the dark, they turn to action, or stop fretting, break out in boisterousness, "cut up," in the evening become electric although in the morning they will be hectic. Even turning up the lights in church, or before the acts in the theater, gives pleasure and tension and habit may make daylight seem commonplace, and the inebriation may come to crave red and other colored or very intense calcium and electric illumination, and the chief pleasure even of evening amusements be found in the light stimulus which to weaker brains or retinas causes depression, or to those over-strained by the fears of night-time, may bring sleep, all the complex and as yet imperfectly explained physiology of which is a direct result of the withdrawal of light. Sleep is in the main the exact biological expression and result of darkness. Although the blind and arctic people maintain the daily rhythm it is largely because their entire organization was formed under the influence of diurnal alternation of day and night. In creatures that hibernate and aestivate the rhythm is seasonal, prolonged and interrupted. Beyond the arctic circle, spring is one prolonged dawn inebriation of light and warmth, autumn a protracted evening depression of nearly all functions of body and mind, when even passion sleeps and sociability is diminished.

The effects of artificial light, while in a measure comparable to those of sunlight, produce certain psychic and physiological changes of so marked a character as to place them in a class by themselves. The data asked for were cases of exhilaration resulting from the overcoming of early darkness by lighting the gas or other artificial illuminants, and the effect of open fires. Of 291 adults answering the syllabus 197 had personal experience of the exhilarating effects of artificial light, and 62 had never noticed any effect. To the 197 personal experiences are to be added 32 cases of observation of children making 229 or nearly 79% of the whole number of cases in which an effect of exhilaration due to artificial light was noted. The degree of this varied from a slight increase in mental or physical activity to cases of actual abandon in children where the effect became distinctly abnormal and interfered with natural sleep. It is evidently conditioned by the brilliancy of the light, as many of the 62 who observed no effect speak of "lighting the lamp," etc., while in all the more marked cases either electric light or gas was mentioned or there was some direct statement in regard

to the more stimulating effect of brilliant light. A few examples of the returns are here given.

F., 18. I have noticed that when it begins to grow dark children become drowsy, but as soon as the lights are lighted they begin to frolic and are more active.

F., 19. I have never felt excited when the lights were lit but I always feel a rise in spirits at the time.

F., 18. When a child I was so very happy when the light was lighted that I would dance around and scream and make a great deal of noise. Even now my spirits rise when the lights are lighted or I am in a brightly lighted street.

F., 18. A little boy whom I have seen a great deal of will sit quietly during the twilight hour. But as soon as the lights are brought in he is anxious for a game of blind man's buff or some other game which requires a great deal of action.

F., 17. I do not remember that when I was a child that the light excited me very much. Now the light in a theater or a brightly lighted room always excites me.

F., 18. As a child I was always extremely fond of having the lamp lighted as I thought it made things much more cheerful but I never became extraordinarily excited about it. At the present time my spirits usually rise in a well lighted room.

F., 25. Different degrees of artificial light affect me very decidedly. Under brilliant electric light I feel full of life and nervous energy. I can think more quickly and am apt to feel buoyant. The effect is like that of a stimulant. Lamplight is to me quieting, a light to study and to read by and in which to look at things calmly, while ordinary gaslight has the most depressing effect and I am rarely happy or lighthearted under it.

F., 18. I think that the presence of artificial light at the opera has had an influence upon me. My body immediately becomes tense, my eyes alert. My interest in everything around me becomes stronger. The same thing is true when there is artificial light in the church and schoolroom.

F., 19. My spirits always rose when the lamp was lighted and were usually higher when for some reason we had more than an ordinary illumination.

F., 19. As a child when the lights were lighted I would begin to frolic and play. Now I feel my spirits rise and feel like dancing, laughing and enjoying myself when a house or room is brilliantly lighted.

F., 18. I observed a child five months old. When it began to grow dark he was fretful and cross but as soon as the lamps were lighted he would grow very happy. He seemed to enjoy looking at the light.

F., 19. One of the greatest pleasures I ever had was upon seeing two clusters of lighted candles (one hundred in each) suddenly brought into a room growing dim with twilight. The people shouted, chattered and laughed for a long time afterward.

F. While making an evening call in a house lighted by electricity a baby was brought in from a darkened room. The child instantly hurried toward the light and seemed so fascinated by it that it was impossible to attract his attention. As his mother changed her position, the child persistently turned toward the light.

The number of returns in regard to the effect of open fires was limited by fact that many of those answering the syllabus had never lived in a house with open fires, and therefore had

no experience of its effects. 149 considered the effect good, 13 bad, and 16 had never noticed any particular effect. The reasons given for considering the effect good are in condensed form, it brings the family together, brings contentment and enjoyment, produces a feeling of coziness, comfort and pleasantness, give a feeling of cheer and homelikeness, induces dreaminess and arouses the imagination. Reasons for an adverse opinion reduce chiefly to the fact that artificial excitement may be harmful in effect. A few examples of the various expressions of this idea are given.

F., 20. The development of evening which comes with fire is not good because it creates artificial excitement which is not good.

F., 19. In some cases I think it is good but in other cases I think people can think better in the dark. Very bashful people can talk better in a dark room.

F., 19. I should think this development would not be good for young children as it would make it impossible for them to sleep quietly.

F., 18. I do not think the excitement which comes with fire is good. I think that the child ought to be so exhausted with his day's play that at night he would not be able to romp and race. I think this excitement is unnatural.

To these data on the effects of artificial light gathered by questionnaire may be added the well recognized psychic effects of artificial illumination on both actors and their audiences, the use of artificial light at social functions even during daylight hours, and the use of illuminations, fireworks and torchlight processions as a stimulus to political enthusiasm during campaigns. For a full treatment of the subject, a study of the use of light in the treatment of disease, psychic effects of electric light baths, etc., would be necessary. The question of how far these effects are to be considered normal and desirable or as an undue stimulus of the nervous system and emotions can be fairly answered only after an investigation of both the physical and psychic elements entering into these reactions, and here, as well as in reactions to sunlight, cases of negative phototropism must be taken into consideration.

The next two topics differ from those already considered in being the result of direct observation, and to the 389 observers must be accredited observations on large numbers of children which cannot be given statistically owing to the fact that the exact number of children observed was stated in but few instances. The questions asked were

What have you felt or observed in children, (a) when a dark cloud passes over the sun; (b) when from a bright field they enter a dark, dense forest or a deep, shady valley; or in both cases, *vice versa*? Illustrations of children's phototropism and love of being in the sunshine, independently of its thermal effects, for light alone. Are there psychic effects of unusual brightness like the sun or new snow?

237 observers note psychic effects of depression more or less marked from the passing of a cloud over the sun. 9 record an opposite effect and 46 no effect; 34 state that they have never noticed. Some of the words used to describe this effect upon children are that the child seems timid, sober, quiet, disappointed, dull, gloomy, spirits droop and face clouds. The depressing effect of passing from a bright field to a dark, dense forest or shady valley was noted by 214 observers and described as depression, awe, quiet, timidity, or that children seem subdued, hushed, thoughtful, gloomy, lower their voices, talk in whispers, cling to each other or to an older person. Only twelve whites and thirty-four colored give a different impression and, in the latter case, thermal effects are probably a modifying influence. The reaction to a bright, cheerful mood on emerging into sunshine is given in every case. 132 report an exhilarating effect from the sun shining upon new snow. 23 (of which thirteen state a weakness of the eyes) dislike the effect. 284 observers report cases of special fondness for sunshine, a large number of children being included in these observations. Only eight cases of dislike of sunshine were noted and three of these were due to weak eyes; the other five "did n't like it because it was too hot." Two liked it sometimes "but not all the time." For the fact that blind children react to these changes in a manner similar to seeing children we are again indebted to Dr. Anagnos.

"The blind are generally susceptible to the influence of the sunshine and gray skies of day but indifferent to the change from day to night.

"A. says that the blind realize whether they are in the dark or light and, on entering a room, know where the windows are and how many, without contact. Keen sensitiveness to thermal effects and to sound will tell them whether a room is lighted artificially or not.

"C. is often affected by atmospheric changes, irrespective of thermal conditions. He can detect slight difference in atmosphere—knows when it is clearing before a seeing person does and feels the influence of coming rain while the sun is still shining and before there is any indication of a change of weather to a seeing person."

The character of the reactions in both adults and children is illustrated in the following extracts from the returns.

Effect of Dark Cloud over Sun. Adults.

F., 19. When a dark cloud passes over the sun I always feel a sense of dreariness. When the sun shines brightly on the snow my spirits always rise and I feel like doing something.

F., 18. Whenever I as a child or even now watch a dark cloud pass over the sun there is a kind of sinking in my feelings and I watch with

great anxiety to see if the sun will come out again and when it does I always give a sigh of relief. Whenever I see the sun shine on the snow I always feel brighter and walk more vigorously.

F., 19. I have often felt a little fall of spirits when a dark cloud passed over the sun, and when it was a decided change as in the dark clouds before a storm I have felt and still feel a sort of awe.

F., 18. As far back as I can remember I have felt a fall in spirits when a cloud passed over the sun. As a child I often felt that the clouds had no business going near the sun. About the age of seven I remember having the feeling that the clouds were teasing the sun.

F., 18. When a cloud passes over the sun it has rather a depressing effect, and when the sun comes out again I seem to feel happier and light hearted.

F., 17. When a cloud passed over the sun I always thought that God was displeased at something I had done and had sent this cloud as a sign of his displeasure. I seemed afraid and would say a prayer. I must have been somewhere between seven and ten years old when I thought this.

F., 17. I have noticed that when a dark cloud passes over the sun children often stop in their play. I noticed only a few days ago a little boy about three years old playing soldier and when a heavy cloud passed over the sun he stood perfectly still and remained quiet until the sun came out again.

F., 20. When a dark cloud passes over the sun, it seems to tend to make my usually bright spirits lower. When a child I thought that God passed this shadow on the sun when some one had displeased him. (5 yrs.)

M., 17. Whenever a cloud passed over the sun, I would chase and throw stones at the shadow and would often run shadow races.

F., 18. I can remember at times having felt a sudden fall of spirits when a dark cloud passed over the sun. I have also felt the same effect in going from sunshine to the darkness of a thick wood.

Effect of Cloud Passing over the Sun. Children.

F., 17. I have noticed when a cloud passes over the sun, a cloud also passes over the child's face and his spirits fall. Children like to be in the sun and often carry their playthings to a sunny part of the room.

F., 16. I have sometimes felt disappointed or chagrined when the sun is hidden by a dark cloud.

F., 21. I knew a little child who used to say that somebody blew the sunlight out when it went behind a cloud and as soon as it appeared again he was very happy and said that some one had lighted it again. He watched the cloud eagerly to see if the sunshine would not be lighted again.

F., 19. I have noticed a momentary pause and hesitancy in children while at play if the sun passes behind a dark cloud.

M., 18. In observing children when a dark cloud passed over the sun I noticed that it seemed to diminish their energy.

F., 20. When a dark cloud passes over the sun children become less sportive. They are quieter and if the sun remains obscured for a long time, they seem to lose life and play with less energy. Frequently a tendency to quarrel seems to arise when before they have been playing quite happily.

F., 19. When a dark cloud passes over the sun I have noticed a look of almost fear upon children's faces. They stop their play and grow quiet.

F., 20. I have often noticed children when a dark cloud passes over

the sun. A look of blank astonishment seems to pass over their faces, and their play seems to lose its merriment.

F., 17. I have noticed that when a cloud passes over the sun, children seem to lose their merriment and do not play as energetically.

Effects of Passing from Sunshine to Dark Woods.

F., 21. I have observed that children on passing into the woods from the bright sunshine become very quiet and want to be near an older person.

F., 20. On entering a dark wood from a bright field, the spirits of children I notice, become subdued, but after they are once in the woods the effect seems diminished. On going from a dark forest to a bright field the opposite effect is produced and the children often become hilarious and throw their caps in the air or skip along joyfully.

F., 20. When children pass from a bright field into a dark, deep forest they seem to feel a sort of fear. They cluster together as if seeking protection from one another. On emerging from the wood they begin to laugh and chatter and run about.

F. Whenever I go from the bright light of the sun into the shadow of the woods a feeling of awe always comes over me.

F., 20. As a general thing as I go from the sunlight into the woods I have a feeling of loneliness.

F., 17. Whenever I enter a dark, dense forest, or a deep, shaded valley I felt I was in a resting place and must be very quiet.

F., 17. When I go from sunlight into the woods my spirits droop and I have a feeling of awe and solemnity. Yet while in the woods I thoroughly enjoy everything around me.

When the woods hid the sun from me I felt as if I were in some sacred place and tried to breathe softly so that no one would hear me.

F. In going into thick woods from bright sunlight a feeling of awe and solemnity comes over me. It does not seem the place for much talking.

F., 18. When I have gone from sunshine into dark woods a feeling of gloom and melancholy has come over me.

When I go into a thick wood a feeling of dread passes over me.

F., 20. When from a bright field, children enter a dark, dense forest they keep close to each other, stop shouting and talk in low voices, sometimes in a whisper. As soon as they come out into the sunlight again they begin to talk louder and are soon shouting at the top of their voices.

F., 21. When children from bright sunshine enter a shady wood, they lower their voices and take each other by the hand. On emerging into the light they run, shout, laugh and play.

F., 21. I have often been in a dark wood with children and they would cling closely to me while I told them about birds and flowers, but as soon as we came out into the sunlight they would all run ahead and play their own games.

F., 19. Children may be running along and playing merrily but as soon as they enter shady woods, I have noticed that they quiet down, walk closer together and talk in lower tones.

F., 19. I often go for a walk with my little sister. As long as we are in the fields she is talking and laughing but as soon as we enter the woods she stops talking and walks quietly along. If she speaks, it is in a lower tone.

Cases of Phototropism.

F., 18. I have noticed children change their place of play when it became shaded, if in the morning they played on the east side of a tree the afternoon would be likely to find them on the west side.

F., 17. A little baby cousin who had just learned to creep always crept toward the sunshine when placed on the floor.

F., 18. I always liked the sunshine and sat in it whenever possible.

F., 18. I always loved the sunshine and felt that if the sun would only shine I could do what I was trying to do so much better.

Examples of children's phototropism frequently given: play on sunny side of room, on sunny side of street, disregard heat to play in sunshine, disregard cold to play in sunshine, babies creep towards the sun on carpet, children always happier and more active in sunshine.

F., 19. I have always had a peculiar liking for sunshine. As a child I always wanted to play on the sunny side of the street.

F., 23. As a child I always loved to sit in the sunshine. I would sit in the sun and read half a day at a time.

In the winter, when the sun was shining on the snow, I always felt happy because everything seemed so bright and cheerful.

M., 13. I like the sun sometimes.

The phototropism of children as well as that of adults and animals has a negative as well as a positive side. It seems to be subject to fatigue. A child who has played in the sun until tired and sleepy does not, as a rule, want to sleep in the sun but craves the opposite conditions. Travellers in Greece often speak of the brightness of the light which, after a time, seems to become positively painful and a gloomy day would be hailed with relief. The construction of the Egyptian houses with their cool, dark, interiors was probably influenced by this negative reaction from the white light and blinding glare of the desert. While in hot climates these reactions are undoubtedly complicated with thermal effects, there is still an influence due to light alone. ¹In experiments with the lower forms of life thermal effects have been cut off by the interposition of a screen which permits the light rays to pass, while cutting off those of heat and both positive and negative reactions have been obtained. ²Greeley notes the insomnia and restlessness consequent upon the long Arctic day, and the necessity of establishing a fixed routine, to insure a proper amount of rest for his men.

Some nervous systems like some complexions are at their best in higher lights than others. Experiments show that blinded frogs tend to prolonged sleep, and the blind live on a very different photometric level from the seeing. Blind children, if not especially cared for, are undeveloped muscularly from their reluctance to move about. Those who see, differ widely in their optimum of light. Very brilliant light has a marked

¹Verkes: Reaction of Entomostraca to Stimulation by Light, *Am. Jr. of Physiology*, Nov., 1899. Parker and Burnett: Reactions of Planarians with and without Eyes, *ibid.*, Dec. 1900. Towle: A Study in the Heliotropism of Cypridopsis, *ibid.*, March, 1900. Parker and Arkin: Directive Influence of Light on the Earthworm, *ibid.*, April, 1901. Verworn: *Physiologie*, p. 434.

²Three Years of Arctic Service, Vol. I, p. 117.

effect on the mentation of the feeble-minded. Some children endure more dark days, darker corners than others without becoming either lazy, somnolent, irritable, or dyspeptic. Average children are so sensitive that a small cloud passing over the sun causes a noticeable depression of spirits, activity, or both. They falter in their play, are less merry, hesitate, pause, and neurotic children often shiver and catch their breath. As the period of reduced light increases to hours or days or as the degree of its intensity diminishes, they speak more softly, whisper or are silent, grow less energetic in their movements, their spirits sink, the quality and quantity of work in school declines, their standards and ideals droop, they are slow and inattentive, all tasks seem harder, the appetite is enfeebled and freaky, pugnacity increases, they are very easily discouraged, huddle, clasp hands, cling about each other or adults, suffer from *ennui*, are prone to collapse attitudes, are lonesome, homesick, etc., but when the sun breaks out, especially on new snow, their exhilaration, noise, activity and joy is boundless.

The returns clearly suggest that adolescence is marked by some change in this respect. Low lights are often craved for the relaxation they bring. A day of rain is a benediction because it relieves tension, and the sombre moods are less shunned and often craved and bring some of the effects of twilight. The complexion effects are now first prized and bring a new aversion to dull days. The effects of both bright and dull light are less purely physiological and more psychic. Young children are often inert and irritable, but pubescents are more consciously depressed and able to give expression to these sentiments, trace their causes, etc.

Psychic Effects of Sun on New Snow.

F., 17. Although the sun on the snow hurts my eyes, it seems as if all the world were brighter and my mood is pleasanter.

F., 19. After a snow storm a change to a sudden outshining of the sun produces a decided change of feeling from one of heaviness to a light and more joyous one.

F., 17. The sun shining on the snow made me feel bright and happy and full of life.

F., 18. When the sun shone brightly on the snow I always felt very happy and wanted to make other people happy.

F., 18. Seeing the sun on the new snow always makes me feel glad to be alive and gives me new courage and inspiration for work of all kinds.

F., 16. I always got into trouble in school when the snow was on the ground. I do not know why but the fact remains.

F., 20. A bright day with snow on the ground has always made my spirits high.

F., 19. I never feel dull or lonesome when the snow is on the ground.

F. The sun shining on the snow makes everything more bright and cheerful.

F., 18. The sunlight on snow had a tendency to make me feel brighter.

F., 18. A bright day in winter with the sun shining on the snow energizes my spirits and I feel stronger than on other days.

F., 24. When I see sunlight on snow I have a feeling of warmth and it seems to act as a tonic to my feelings in general.

F. A bright snowy day in winter always exhilarates my spirits.

F., 19. The brightness of the snow in sunshine always made me feel happy.

Summarizing briefly the effects of shade and sunshine, it is evident that there is a close connection between the general feeling tone and the amount of direct sunlight. With the obscuring of the sunshine a chill seems to fall upon children, there is a drooping of the spirits and play is less vigorous and spontaneous. While the effects are more noticeable in children, adults are also affected by photometric changes even those of short duration; sunshine, cheerful spirits and vigorous activity of mind and body seem co-ordinated, while gray skies and deep shade produce depressing effects, more or less marked, and a lowering of the mental and bodily activities.

The effect of the absence of sunshine for longer periods of time than those just discussed, on both mental and physical conditions, is well marked. The questions were

Have you observed any effects of a dark day or a series of them in school or elsewhere upon children or yourself; effects upon appetite, digestion, complexion, quantity and quality of work of mind or body; does a series of dark days bring you some rest; are pupils in the dark corners of the schoolroom more active or sleepy than those in its brightest part?

154 adults report personal experience of feelings of gloom depression and restlessness in addition to observations of children. 53 state that they have never observed any difference in their feelings. In 86 cases appetite was lessened while 54 report an increase in appetite which seems to be somewhat morbid, points noted being a desire to eat all the time, craving for sweets or unusual articles of food, craving for highly seasoned food, etc. 111 state that their appetites are not affected by weather conditions and 57 had thought about it. Digestion follows very nearly the line of appetite though no cases of improved digestion are reported as corresponding with increased appetite. The effects of dull days upon mental and physical work differ, the preponderating effect upon mental work being bad while upon physical work the good and bad effects are nearly equally divided. A direct comparison of results here is of interest.

Mental Work
as affected by dull days.

Worse	80%
Better	17 $\frac{1}{4}$ %
Less	79 $\frac{4}{5}$ %
More	18 $\frac{1}{5}$ %
No effect	2 $\frac{3}{4}$ %

Physical work
as affected by dull days.

Worse	38 $\frac{4}{5}$ %
Better	35 $\frac{1}{5}$ %
Less	35%
More	50%
No effect	15%

These results show that for a large proportion of adults mental work is poorer in quality and less in quantity than on bright days while the effect upon physical work is variable. In the case of children the testimony is almost unanimous as to the deteriorating effect of dull days in the schoolroom. Some of the phrases used repeatedly to describe these effects are dull, peevish, cross, irritable, mischievous, hard to keep in order, do not concentrate attention, fidgety, sleepy, low spirited, idle, hard to teach, hard to interest, difficult to gain their attention, slow of comprehension, unresponsive, etc. These effects upon both children and adults are brought out in the following quotations from returns.

Effect of Dark Days on Adults.

F., 17. A series of dark days makes me gloomy and fearful as if I would cry out against the dull atmosphere. It is more difficult for me to keep my attention on my lessons and I feel sleepy.

F., 17. A series of dark days usually makes me very dissatisfied and cross. I cannot do so much studying but can do more housework and sewing and like to do it. It takes me longer to learn things on dark days, but I generally learn them usually well.

F., 18. On a dark day or during a series of dark days I never could study as a child and I cannot now.

F., 18. It is harder for me to concentrate my attention on my work on rainy days than on clear ones, but I cannot see any difference in the quality of the work.

F., 24. A series of dull days affect both the quantity and quality of my work. I have observed that I can do less school work on a dull day, but more of such work as sewing, fancy work and manual training. On a clear day I can do better school work than on a dull day.

F., 18. On dark days I cannot do as good thinking as on other days, but I can do manual work and pleasant reading with more contentment than on other days.

F., 17. On a dark day I can work a great deal better than I can on a clear day. My brain seems clearer and there are not so many outside things to attract my attention. Dark days make some children listless and inattentive.

F., 18. I have always noticed that when there was a dark day or a series of days I have not been able to put much attention on my studies. From about nine years old until now I have often noticed that on dark days my work is not so good and I cannot do as much as on a bright day.

F., 18. During dark days my work seems more difficult and irksome. I feel low spirited and homesick. The quantity and quality of my work is influenced. I find that I can neither do as much nor do it as well as on bright sunshiny days.

F., 18. Dark days always make me tired and depressed. I have a tendency to yawn all the time. I do less work and work that is not very good on dark days.

F., 22. A series of dark days always made me sad and gloomy. When a child I was always restless in school and did not do as good work as on clear days. I have noticed that children are more restless and their work is not up to the standard on dull days.

F., 17. Until the last two years I have been depressed on dark days and did not do as much or as good work. Now it is on dark days when

I cannot be in the open air that I put my mind on my work and accomplish more.

Effect of Dull Days on Children in the Schoolroom.

F., 23. I have noticed that on a dark day in school it is hard to get the interest or attention of the children and everybody seems depressed in spirits. I can never work as well on a dark day as on a bright one.

F., 18. A series of dark days always inclines me to make the children stupid; they do not take the interest in their work that they do when the sun is shining.

F. On a dark day in school I have noticed that it is harder to keep the pupils interested, harder to make them understand and harder to keep them out of mischief.

F., 19. I have observed that on dark days children are inclined to be troublesome or dissatisfied with things in general.

F., 19. A series of dark days in the school I used to attend had a remarkable effect upon both teachers and pupils. The children became cross and irritable and inattentive, while the teacher seemed depressed and cross.

F. A number of dark days in school or even one dark day, tends to dampen the spirits of the children. Things seem harder on such days and children do not seem to try so hard as on bright days. The quantity of work is lessened by a dark day and the quality is also affected. We do not seem to take the same pains with work as on other days but seem to think almost anything will do. We seem to lower our standard at such times and hence it is not a good time for delicate or trying work.

F., 19. I have observed that a series of dark days generally makes children cross. One little boy whom I know always becomes restless and irritable if we have rainy or cloudy days in succession. A series of dark days depresses me greatly. I do not know that it affects the quality of my work but it takes me longer to do it.

F., 22. I have noticed that on a dark day or a series of dark days pupils are more restless and lacking in attention. I am never so hungry on a dark day and my digestion is not as good. My complexion is sallow and my face seems to lose plumpness.

F., 18. I have noticed that on a dark day or a series of them, that children in school are more restless and it is harder to keep their attention. I always feel rather dull and never do as much or as good work as on a sunny day.

F., 22. A series of dark days makes children restless in school. They do not apply themselves to their work so well.

F., 19. If the day was dark my lessons in school were always a bore and I longed for something strange or startling to happen. In most cases I think pupils are depressed on dark days.

F., 23. I have frequently had occasion to observe the effect of a series of dark days in the schoolroom. The children become restless, hard to control and new devices must be used to hold their attention. Lessons are not as well learned, tendency to sulk is more frequent and the atmosphere of the schoolroom seems depressing.

F., 18. A dark day or a series of dark days has an ill effect on the children. They become depressed, worrisome and tired and very often the teacher undergoing the same effects of depression, makes the schoolroom anything but an ideal place.

F., 21. On dark days children are cross, restless, uneasy, dull, slow of comprehension, peevish, inactive, irritable, do not respond readily, are sleepy.

F., 19. I have noticed that a dark dismal day makes one feel dull and without animation. I have particularly noticed this in second year school children.

Effects upon the complexion are described as follows:

F., 20. My complexion is dark and sallow on a dark day.

F., 17. My complexion is much darker and duller on a rainy day (brunette).

F., 18. I have noticed that after a series of dark days, I usually have a very sallow complexion though ordinarily I have a good deal of color. I have also noticed this to be true of other people.

F., 18. After a series of dark days my complexion is paler than usual.

F., 18. I become paler and dark rings form under my eyes.

F., 19. On dull days my complexion seems deadened and it is not generally the same color unless the day be damp. A warm dampness has a tendency to produce higher color and increase freshness.

The effect of a dark day on many people is to make the complexion pale and the eyes duller.

The effect of sitting in dark corners of the schoolroom is very similar to that of dark days. 75 per cent. of the observers report that children seated in dark corners are less active mentally; a number add to this statement they are more active in mischievous ways while others describe the effect as sleepiness or dullness; one observer considered that children were more studious while the remainder had noticed. Some of the direct statements are:

F., 22. When seated in a dark corner of the room I could not do as good work. I noticed the same thing with my pupils while teaching and always gave the duller pupils the benefit of the light. I found they did better work in a good light.

F., 17. In my fifteenth year I sat in rather a dark part of the schoolroom and was then more inclined to mischief and less inclined to study than I have ever been before or since. I managed to study my lessons but my work was not up to the average.

F., 18. Pupils in dark corners seem sleepy and dull.

F., 20. Pupils in dark corners are less active in school work and more active in mischief.

Only 14% of those answering the syllabus found any restful effect from dark days while 39% report an opposite effect, the remainder never having noticed any result in either way.

Effects on the blind similar to those on seeing children are reported by Dr. Anagnos, who says:

"The effect of dark days is to cause depression; they affect quantity and quality of the work. (C. adds, "and appetite.")

"A. found early in her teaching of totally blind children that they wanted the shutters open—especially at the top—and the light admitted freely. If sunshine flooded the room, so much the better.

"A. thinks long days of light give a feeling of lightheartedness; but neither B. nor C. gave indication of having such a

sensation and could not seem to abstract thermal conditions from their consideration."

Arctic explorers have noted both the mental and physical effects of long continued absence of sunlight during the Arctic night. An extract from Lieut. Lockwood's diary says: "The effect of continued absence of sunlight is very marked in the complexion of all the men as well as in their vigor. They are as blanched as potato sprouts in a cellar. The moral as well as the physical influence of sunlight is very soon seen after the sun's reappearance, the middle of Feb." Lieut. Greely speaks repeatedly of the mental irritation and depression which affected the entire party while at Ft. Conger and as comfortably situated as was compatible with Arctic conditions. The most marked symptoms which he notes were tendency to insomnia and the reverse, indisposition to exertion, irritability of temper and mental depression. He speaks in his own case of the difficulty of limiting his sleeping hours to a reasonable number, of applying himself successfully to continued mental work. He adds, "While free from mental depression, insomnia and feelings of lassitude which characterized some, yet I was at times affected by irritability of temper which it required a continued mental struggle to control. But few men were exempt from this symptom."¹ In speaking of the effects of Arctic night upon the complexion he describes the faces of the men as gradually acquiring a pale yellowish green color the full extent of which was not clearly revealed until the return of light. Dr. Kane and Nansen describe similar effects upon both the disposition and physical organisms of their men. Effects² even more pronounced than these and of a more serious character began to be observed at Ft. Conger about the middle of December, as at that time, some of the men under the influence of continued darkness began to show indications of mental disturbance. Appetites failed, and signs of gloom, mental irritation and depression were all increased, the Eskimo being more seriously affected than the Americans of the party and one of them wandering away without food or proper clothing during a fit of mental alienation. Though thermal effects must be taken into consideration in this connection we find Stanley under conditions varying as widely as possible,³ describing with great vividness the sullen gloom and despair which settled upon his men during their long march through the great forest and the reaction when at last they emerged into the light of day. "They held their hands far out yearningly toward the superb land and each looked up into the bright blue heaven in grateful wor-

¹ Three Years of Arctic Service; A. W. Greely, Vol. I, p. 117, 154.

² *Ibid.*, 167.

³ In Darkest Africa; H. M. Stanley, Vol. I, p. 282.

ship. After they had gazed as if fascinated, they turned their heads to the forest and shook their clenched hands at it with gestures of defiance and hate." Though these reactions are extreme and the product of unusual or abnormal conditions, they are, nevertheless, fully in accord with those of a normal type furnished by the returns. Reactions to light are in the direction of life, health, activity and moral growth; those of darkness in the direction of mental and bodily inactivity and, unduly prolonged, show indications of tendencies toward moral deterioration. The plea for well lighted schoolrooms scarcely needs reinforcement at the present day but it becomes a question of practical interest whether the morals as well as the physique of the dwellers in tenement houses might not be improved by a more liberal allowance of sunshine. Somewhat apart from the purpose of this study, but of psychological interest, would be a study of the use of light in the great masterpieces of literature. Dante revels in the use of light. It pervades the *Comedia* in every form, and to the contrasting use of shadow and darkness the *Inferno* owes many of its terrors. Throughout, in its moral significance, light is always the symbol by which the soul rises by successive stages while the souls in the lowest depths of hell are consigned to utter darkness. This is poetry and not science, yet Dante by that same marvellous insight by which he read the inmost secrets in the hearts of men seems to have anticipated the facts which point to the conclusion that these are mental, moral and physical effects of light and darkness which constitute no mean factor in the development of the individual and the race.

A PLEA FOR SUMMARIES AND INDEXES.

By E. B. TITCHENER.

When Wundt began the publication of his *Philosophische Studien*, certain critics complained that, not content with giving the results of his investigations, he needlessly inflicted on the reader a statement of the methods whereby the results were obtained.¹ At the present day, such criticism strikes us as almost comical. We require from an experimenter, as a matter of course, that he give a full account of appliances, method, sources of error and safeguards against error, number and character and training of observers, and what not: an account so full that we may be able, if we wish, exactly to repeat his work in other laboratories. As we read the earlier literature, we sigh for the *Rohtabellen*; it is the insufficiency, not the surplusage of detail that strikes us. Under the best conditions, experimental psychology is difficult. We want to be assured, then, before we admit a set of new results into our psychological system, that the conditions of their attainment *were* the best.

This general tendency, to a demand for and a supply of detailed information, has been furthered in a special way by the historical development of the science, by its gradual swing from quantitative determination to qualitative analysis. As late as 1893 so good an experimenter as Merkel could write a tirade against the admission of qualitative factors into method work.² Now, only ten years later, we seem, as we read him, to be reading the language of a different epoch; his standpoint is one that we have long outgrown, and almost forgotten. It is, I think, no exaggeration to say that some of the earlier Leipzig researches, if they had been reported as researches are reported to-day, would have been drawn out to three times their present length.

At any rate, there can be no doubt that our experimental literature is rapidly increasing in bulk,—not only in the sense that more investigations are being published every year, but also in the sense that the single papers are becoming longer. This increase of length throws a very heavy burden upon the shoulders of the psychologist who tries to 'keep up' with all

¹W. Wundt, Schlusswort zum ersten Bande: *Phil. Stud.*, i, 1883, 616.

²J. Merkel: Die Methode der mittleren Fehler, etc. *Phil. Stud.*, ix, 1894, 196 f.

phases of his science. The burden will be cheerfully borne, since its incidence means, without any question, that experimental psychology is really advancing. But there is no reason why it should be made unnecessarily heavy. And my point here is that writers of monographs are apt to make it heavier than it need be.

For one thing, there can be no doubt that many published papers would be improved by condensation and curtailment. The swing towards qualitative detail has gone too far; the authors lack perspective. However, I do not wish to stress this fact now. I propose rather that we increase—by a little—the length of our monographs: that we make it a rule (1) to prefix a sectional table of contents to every article that runs say, to 25 pp.; (2) to write out, at the end of the article, a summary of its contents, with page or section references; and (3) to supply the editor of the magazine in which the article appears with an analytical index. These proposals can hardly be considered iconoclastic. Tables of contents are prefixed, as it is, to many of the longer papers in this *Journal* and in the *Zeitschrift*. The trouble is only that they are not prefixed to all; and that, when they are printed, they are not sufficiently detailed. Summaries, again, are appended perhaps as often as not. They are likely, however, to be over-condensed, and they do not, as a rule, give back-references to the body of the article. How much a summary can accomplish at its best is admirably shown by Meinong's *Zusammenfassung* (3½ pp.) of his papers (some 160 pp.) on Weber's Law.¹ I have heard it seriously objected that, if the summaries are made too good and too full, readers will attend to them and skip the articles. Well! if the reader is abstracting for a magazine, and merely reprints the summary, the author has surely no ground for complaint; it is his own summary that is reprinted. If, on the other hand, the reader is reading for his own benefit, the objection becomes nonsensical. No serious student would allow himself to think, still less to print, on the basis of a reading of summaries. What the summary does is to give the reader his bearings within the discussion, by way of direction from the author himself. The author is again advantaged, as well as the reader.

The proposal of an analytical index is not, I believe, sanctioned by precedent. Its advantages are, however, clear enough. The author is fresh from his work; he knows its details better than any one else. Volume indexes are now prepared, as a rule, by a business editor, or by his clerk, and neither editor nor clerk is necessarily a psychologist. That our

¹ *Zeits.*, xi, 1896, 399 ff.

volume indexes are as good as they are reflects great credit upon their makers. But they might be incomparably better. And think of the *Studien!* Twenty large volumes, and no index at all! Doubtless, we shall presently have a general index, published as a *Supplementband*; and, doubtless, the general index will be inadequate. Why should not the authors, who can do this work well, and do it with little effort, be expected to hand in an index along with their MSS.?

These things have been in my mind for a long time. But I have been prompted to write out my suggestions by the recent appearance of Vol. i, of the Harvard Psychological Studies. Here is a volume of viii + 654 pp. It contains 16 papers; an average of 40 pp. to a paper. It is not a loose collection of essays; it has a general editor, who declares that "there is no absence of unity in our work;" the work itself has been done, all of it, "by well-trained post-graduate students." Yet there is no single instance of a sectional table of contents. There is no index of any sort or kind. Only six of the sixteen papers have formal summaries. Suppose, then, that the reader goes to the book, not to read some special paper for some special purpose, but with a general question,—as I personally have to go to all psychological books, just now, to see if they contain any reference to mental measurement, and the metric methods, and the blank experiment, and so forth? The title of a paper is no indication; a man may be discussing the immortality of the soul, and yet have his fling at minimal changes. The only thing to do, unless one wants to have these "well-trained post-graduate students" commenting later on one's bibliographical ignorance, is to turn over all their six hundred pages, and see what one finds. I suggest that a very small expenditure of time and trouble on their part, and the printing of some 25 additional pages, would have made the volume indefinitely more valuable to the working psychologist.^{1 2}

¹ I may use this opportunity to protest, also, against the mode of publication of Stumpf's *Beiträge zur Akustik und Musikwissenschaft*. These *Beiträge*, as is well known, are to replace the promised third and fourth volumes of the *Tonpsychologie*. Heft 1 contains Stumpf's paper on Consonance and Dissonance: new matter, for which one gladly pays one's Mk. 3. 60, Heft 2, of 170 pp., contains just 3 pp. of new matter; the remaining 167 are reprinted from the *Zeits*. Why should one have to pay Mk. 5 for these 3 pp.? Heft 3, of 146 pp., contains 90 pp. of new and 56 of reprinted matter. Of course, not every one who takes the *Zeits*, takes the *Beitr.*, and conversely. But the experimental psychologist has to read both; and experimental psychologists already have access to the *Zeits*. The present intermixture of new with old material argues at least a lack of consideration on the editor's part.

²As the matter of summaries and indexes is one in which all work-

ing and publishing psychologists are interested, Professor Titchener has invited me to add a note to his paper expressing my views on the subject also. I am happy to do so, for though the matter is a mechanical one and seemingly quite insignificant in comparison with the quality of thinking embodied in the paper, it is just these mechanical aids to work that make more and better work possible. It is a case of the telephone and typewriter over again. I may say then in one word that I heartily concur in all that Professor Titchener urges with reference to the importance of table of contents, summaries and indexes. I do not concur, however, for a moment in his even temporary omission to "stress" the fact that psychological papers are for the most part unconscionably long. A hundred pages are often taken for saying what ought to be said in twenty-five, and could be said if they were confined to a statement of points really demonstrated and essential conditions.

E. C. SANFORD.

NOTE ON MOON FANCIES.

G. S. H.

Principal E. H. Russell, of the State Normal School of Worcester, has kindly sent me the replies of one hundred and eighty-four normal school pupils and recent graduates, mostly girls between the ages of eighteen and twenty-two, on their own early and present feelings and ideas concerning the moon, the following note on which is not without value and interest as supplementing Dr. Slaughter's¹ recent article.

Of these persons eleven specified in their early childhood grave and prolonged feeling that the moon might fall, and several dared not look at it because it made this fear painful. Sometimes it took the form of fear of conflagration if it touched the earth, for it often seemed a ball of fire, but more often they feared that it would crush them. In seven very distinct cases the person in the moon was a woman with a child, thought to be the Virgin and the Christ Child. In one case the notion long persisted that there were four moons appearing in turn. Later, when phases were learned, the idea still persisted that it was only another way of speaking of four moons. Sometimes, when it was little seen, it figured largely in the imagination. Some who had heard it was green cheese insisted that it was a yellow one, or perhaps hoped it was a Swiss or Dutch cheese and wanted to go there because they liked cheese, etc.

About forty specified that they long believed that the moon moved, followed them, or often ran this way and that toward or away from it, and sometimes were terrified that they could not escape it, but it went wherever they did. Not a few identified it with God or His all-seeing eye, which could not be escaped. For some this pursuit was friendly and protective, but to more it was inimical or painful.

A few saw a large variety of small animals in the moon. Sometimes they thought them immune to fire, which was held to be the moon's substance. That the moon could see and had some kind of consciousness of the child's acts was a very prevalent idea. To some the moon gave great ecstasy. They would shout and cry out when it was seen. One called it a man, but the crescent moon a lady. More than a score won-

¹The Moon in Childhood and Folk-lore. *Am. Jour. of Psy.*, April, 1902, Vol. 13, pp. 294-318.

dered why it did not fall down, it seemed so heavy. Fifty-six thought there was a man in the moon. A few persistently interpreted it as a face looking down at them. Others still have a great desire to go toward or get near the full moon and strive, though quite often in vain, to correct their childish ideas of its distance and size.

Several always felt cold and shuddery at sight of the moon. One thought it must have white grass on it, while several thought it silver. Some were for years fascinated by it; these long for its appearance, never tire of gazing at it, and would move their bed to the window that it might shine full in their face at night. One remembers first noticing the moon at the age of five and crying, "Why there's God. Halloo, Mr. God." Several have hovered between pleasure and pain as to what would happen if the moon caught them. Six thought it full of fairies. One said, when it looked red, the moon weather was so warm that the fairies came out of it and turned into stars. A few can remember actually reaching for the moon and longing to get it, touch it, hold it, see what it was made of, and taste it. A few girls fancied the moon escorted them on their evening walks as a protector, if not almost as a beau.

Many record their experiences in trying, some with and some without success, to trace out the features of the man they heard of in the moon, devising schemes of how he got there; wondering what he did; sympathizing with his loneliness; imagining friends or a family for him; wanting to help him; and even feeling tender sentiments toward him.

One heard as a child that if anything bad happened, the moon ran away and hid, and still feels relief at sight of it. Several want to do something extraordinary whenever there is a moon, especially a full one. Three thought the moon was one of the parents of the stars and cared for them. A very few always shudder at and still have a dread of seeing it. Two thought the moon was God's house in which he lived. The moon more often has sight than any other sense; it watches all that goes on and if it could speak could tell us of distant friends. To some it can also hear. One child, thinking the moon was God's face, would always scrutinize it to find in it expression of approval or disapproval of her acts. One boy of five thought a man went about with a wagon load of moons hanging them up, and the idea that each locality has a moon of its own is widespread.

One always thought the moon a large glassy cent and could dimly see a man's face through it. Some developed ideas about the moon eating the cheese, or think the moon full of mice. Young children often think the moon is lighted and hung out, perhaps with strings. Some distinctly change, either with or

without effort to do so, from seeing a whole man's figure to seeing a simple face, or *vice versa*. One always laughed at the moon, thinking it funny. A few think it rolls along the sky. One was long curious to know how he viewed things on earth. One, "despite the geographies," believes it to be about the size of a dinner plate. To a very few, it is an opening in the sky through which the sun shines. To a few God has put it in the sky to protect children. One always feels as though a friend were dead on seeing it, although generally the sadness is rather enjoyable. One traces the details of heaven in its shadows, thinking it is heaven. To some it seems always smiling; to one it always suggests death. Two are troubled because they always see a woman, not a man in it. One thought it a grown-up star and wondered it did not twinkle.

Several are greatly fascinated by the wonder and awe of it or take great satisfaction in simply gazing at it for a long time. To one the man was Santa Claus. One was long sceptical when told that her brother in New York saw the same moon. One saw in it great numbers of people and animals that had died. One was greatly alarmed and grieved because it seemed so pale one night. Very often the transition from fear to love is noted. Several think there are two moons, a pale one seen by day and the yellow one by night. Some think the crescent hungry and the full moon plethoric with food. Some run races with it. One long thought it a policeman watching people; one that the full moon belonged to Worcester and the other phases were moons from near by cities here on a visit. Loneliness as well as depression are sometimes the chief feeling. In some it provokes almost nothing but reverie and silence. A few have an *eclaircissement* or else are sceptics from the start in maintaining that there is no man in it. Two, learning that it was cold and dead, with no air or water, watched it and found it so merry that they felt it a hypocrite practicing deception.

Some play hide and seek with, and a few talk to it. One specifies greatest dread when it is near the horizon, because it is so close. Perhaps it is wild as well as fiery and better kept behind one than in front. One imagined a star party with the moon an honored guest. One sees an old woman sitting and telling stories to children at her feet. Another sees two pails of water in her hand. Another finds a pile of sticks on fire. For one a moon-glade on the water is a path to it. Some dread to go to bed on a moonlight night, because it seems losing so much. For several the moon shines when people are good, and when it is away they are bad. One can never gaze on it if she has done wrong that day. As she was going home one night after a terrible storm, the moon shone from behind a cloud just in time to prevent her from falling into a deep hole. This

she thought a special providence. She can still not bear to hear of a crime committed on a moonlight night.

One thought the moon and the sun the same, only it is paler at night because of darkness. One always cries at the sight of the moon, she knows not why. Another associates it with water. The moon came out just as one child was hiding, and she was terrified to find herself discovered. Some the moon soothes and gives a sense of ease, calmness and comfort; it is a great sympathizer. Several speak of being deeply affected by the moon in the early teens and of spending much more time in watching it. On hearing some one say, the moon is too beautiful to talk about, it long seemed to a young girl sacrilegious to speak of it. One saw a woman in a ruffled skirt and sunbonnet sitting in the moon, which was her house, lit so brightly that she could not see to walk about because the light was in her eyes.

THE SIMPLICITY OF COLOR TONES.

By I. MADISON BENTLEY.

The existence of composite colors has long been a matter of dispute. Among psychologists, no less than among artists and the laity, the contention has frequently been made that red and blue are pure, elementary, unmixed colors while orange, yellow-green and purple are mixed, derivative, composite.¹ If we ask—not how a given color is produced, but—what one actually sees in a color—a single quality or a plurality of qualities—the problem becomes strictly psychological and must be worked out under psychological methods and in psychological terms. Let us see if the problem is soluble.

The place of any color whatsoever in the general scheme of visual qualities is determined by three factors; (1) color-tone (as red, greenish blue, violet), (2) brightness, *i. e.*, relation to black and white, and (3) saturation, *i. e.*, distance from gray of an equivalent brightness. Our special question arises in connection with the first factor; color-tone. Are all tones equally simple? or are some tones simple and others complex? Current color terms do not help us to decide. The names of many 'intermediate' colors suggest, it is true, a plurality of components; *e. g.*, 'yellow-green' and 'orange-red.' But we are not warranted in drawing the inference that these compound terms really cover mental complexes. And, moreover, the appeal to introspection has never been entirely satisfactory. Psychologists seem to be unable to agree on what they 'see' in the yellow-greens, the oranges and the purples; whether a single quality or more.

Look for a moment at the general system of color-tones, as arranged, let us say, around the base of the color-pyramid. The first thing that strikes one's notice is the presence of two

¹That the 'composite' theory has not lacked authoritative support is shown both by Hering's contention that yellow makes a much stronger impression of simplicity than does violet (*Ueber Newton's Gesetz der Farbenmischung*, 1887) and by Goethe's conviction,—a conviction that was shared by Sir David Brewster—that he actually 'saw' blue and yellow in green. (*v. H. Helmholtz, Physiol. Optik*, 2nd ed., 380.) Even Helmholtz, while he criticizes earlier writers for confusing mixed pigments and mixed colors, himself falls into the error of believing that nearly all colors are analyzable into simpler elements (cf. *Sensations of Tone*, 1895, 64).

major groups. The one contains the reds and the yellows; the other, the greens and the blues. Within each group there is a special kinship that marks off the one group from the other. Within each group, again, we find two minor groups each of which forms a qualitative continuum. Toward the ends of each major group lie parts of other continua which are completed by joining these groups at both ends (Y to G and B to R). Thus we have four continua, R to Y, Y to G, G to B and B to R. Each continuum is comparable, introspectively, with the black-white series. These qualitative continua differ from intensive series in that they lie between unlike qualities instead of between zero and a maximal value. In a 'perfect' intensive series, there is no qualitative change and no fusion of intensities—only variations in distance from zero; in a 'perfect' qualitative series there is only qualitative change—neither change of intensity nor admixture of unlike qualities. Such a perfect series of the latter type may be represented by the tonal continuum.¹ In this series, all members are equally simple. No tone contains a lower and a higher; although a pitch is more *like* its near than its remote neighbors. Introspection on this point is unambiguous. But are the color continua such 'perfect' qualitative series? This is the crux. Intensive differences among the visual qualities offer no difficulty. They may be ruled out. The difficulty lies in the alleged complexity of the 'intermediate' colors. Does an orange 'contain' both red and yellow? If it does, can it be factored into a red and a yellow? or is 'orangeness' left over from the analysis? The facts of color mixture must not be adduced as argument. Of course, O is 'produced' by mixing R and Y; but this fact is irrelevant.² Nevertheless there does seem to be an introspective difference between orange and yellow or orange and red. Two points must, I think, be conceded to the complexity argument. First, there *is* a peculiarity about the four qualities, R, Y, G, B.; and, secondly, all other tones 'look like' some one pair of these. The peculiarity consists in the fact set forth above; the fact that these four qualities are the natural termini of the four continua. Black and white are peculiar in the same way. We can even conceive that black should be the initial member of two continua (instead of one) as R is. In regard

¹ The fact that a constant physical intensity does not give a constant mental intensity is not important.

² I have been at some pains to question artists on their distinction between simple and compound colors. From their own introspective accounts and from the fact that they class green with the compounds, I conclude that the distinction rests solely on the method of pigment-mixtures. Blue is a 'pure' color because it is produced by a pure pigment; green is compound because it is made by the mixture of yellow and blue pigments.

to the second point, the likeness of intermediates to termini is not a valid argument for the complexity of the intermediates. For (a) orange is 'like'—let us say—twelve other qualities, orange-reds and orange-yellows; but it would be absurd to contend that orange is composed of twelve elements or that its elements are indeterminate. Again, (b) any one of the termini, *e. g.*, red, is 'like' a score of oranges and purples, but the argument from similarity deftly avoids the implication that red is compound.¹ Finally, (c) the similarity argument would forever debar science from having 'elements,' since it is impossible to find anything so simple that it is unlike everything else in the world.

What support is left, then, for the alleged complexity of the intermediate colors? There is left the argument that the color system presents four color types which, however we combine them, are never augmented. We get always combinations; never new types. This is a fair psychological argument and must be taken account of. But though it is true that we never get new types, it does not follow that we get no new color qualities.

Granting this, however, it may be answered that the new colors are fusions of the old and, therefore, complex. This brings us face to face with the ultimate question of the criterion for elementariness. Elementariness may be either psychological or psychophysical. A psychophysical criterion would give us, in this case, either simplicity or complexity according as we maintained allegiance to one theory of visual sensations or another. The Helmholtz theory, *e. g.*, would make every visual quality complex as depending upon a plurality of nervous structures. The Hering theory has four primary colors (if we leave out black and white), each of which depends upon a specific function or a specific structure. For the mediation of all other color-tones, more than a single type of struction is required.² Psychological criteria of simplicity are often difficult to apply; but they are vastly to be preferred, within psychology, over psychophysical criteria. One analyzes introspectively so long as one can think a quality or a group of qualities as exist-

¹The argument from similarity is stated thus by Stout. "The respect in which blue and blue-green are seen to resemble each other when compared is different from the respect in which green and blue-green resemble each other when compared. This appears to me a sufficient reason for inferring complexity in the blue-green." (Manual, 149.) But the argument works as well for blue compared, on the one hand, with blue-green, and, on the other, with violet or for red compared with purple and with orange.

²If the simplicity of all color-tones be admitted, the stock argument against the Hering theory for selecting a *bluish* green for its Urgruen and a *purplish* red for its Urroth loses its strongest support.

ing apart from its context. When the element is reached, the object of attention refuses to be thought further as object and context. Attention is no longer able to pass from point to point without apprehending material already separated off. Take the sensation purple. Does one get the element red within the purple? or does one pass beyond the purple and institute a comparison of similarity between the purple and some imaged or observed red? The latter, surely.

But, it may be answered, to compare the two qualities and to judge them similar is precisely the necessary first step in analysis. A mental complex is dissected it may be urged, by determining its likeness to a plurality of other contents. This view of analysis is current in the literature. It can, however, be said that comparison leads invariably to analysis only on the assumption that similars are always complexes, possessing identical parts. But this assumption leads to a logical absurdity. If elementary things are, in no sense, alike, they cannot be compared; cannot, therefore, be considered even different, neither can they be brought together into a system. No: comparison is essential to *classification*—the relative positions of red, blue and purple in the system of color qualities, *e. g.*, is determined by comparison—but classification and *analysis* are quite different processes and yield wholly unlike results.

Thus we see that the arguments both from the four visual types and from the twofold resemblance of the intermediate colors are insufficient to prove the complexity of visual qualities; while we find ample grounds for the belief that one color-tone is as simple and as ultimate as another. At the same time, it would be difficult to find a more illuminating instance of the essential difference between psychophysical and psychological problem and of the necessity for distinguishing mental analysis from the objective simplification of physical and physiological factors.

CHILD STUDY AT CLARK UNIVERSITY.

AN IMPENDING NEW STEP.

By G. S. H.

It is now nearly nine years since the first child study questionnaire was printed at Clark University. Now over one hundred have been issued and over fifty books and articles, entirely or in part based on returns from these questionnaires, have been published. Only a few questionnaires have been entirely abortive. Many of the best papers have needed a second set of questions and data, quite a number of topics already in have not yet been worked up, and a number are in various stages of preparation. In connection with the new quarters of the psychological department, two large rooms have been set apart for this work. In one computations are made and data compiled, and literature gathered; and for the other a special library of child study, including the following questionnaires and articles as a nucleus, and special literature on each important topic is begun.

Another new step will be taken in the coming Summer School as indicated in the following announcement. "Dr. Hall will offer a course of daily conferences on child study, its methods and results. This will be a distinctly new course on probably about twelve topics. Each member will be furnished with syllabi and be expected to do some definite work in both standard topics and others now under investigation to bring out the logic of this work, its errors and defects."

Next year in the regular course this work will be expanded in a series of weekly exercises throughout the year. This will cover nearly forty of the chief topics, and much attention will be paid to the discussion of the sources of error, the different methods and their evaluation, and the many new problems in logic suggested.

More elaborate bibliographies on special topics may be published from time to time throughout the year.

In connection with the gift of \$1,000 by Mr. Arthur S. Estabrook, of Boston, and the grant of \$2,000 for this work from the Carnegie Institution, a competent and well trained research assistant has been engaged, all of whose time is devoted to working up data and to assisting students whose theses or other work happens to fall in this field.

Finally, methods of co-operation are now being agreed on between this line of work at Clark University and a number of select institutions elsewhere, whose professors and others have already taken great interest in or shown special aptitude for this work. This, it is believed, will secure data of the required kind and amount.

At first child study passed through a period of criticism such as few new scientific movements in the modern world, save evolution alone, have had to sustain. It had, too, a host of camp followers who had little conception of its meaning and no idea of its severity of scientific method, and who offered many very vulnerable points of attack. Some four or five years ago, when the critics were loudest and most aggressive, many superficial observers thought the movement dead. But it has steadily spread to department after department. In insanity it has given us the new studies of dementia præcox; has almost re-created the department of juvenile criminology; furnished a new method of studying the most important problems of philology (as illustrated in the one sample bibliography on this subject appended); has revolutionized and almost re-created school hygiene; made adolescence, a strange word ten years ago, one of the most pregnant and suggestive for both science and education; given us the basis of a new religious psychology; and laid the foundation of a new and larger philosophy and psychology of the future, based not on the provincial study of a cross-section of the adult mind, but on a broad, genetic basis. The few able psychological and philosophical professors, who still refuse to accept it, as Agassiz did evolution, will not escape the same kind of criticism meted out to him.

The importance of this new movement it is hard to overestimate. It has brought a new and large hope into a field that was in danger of lapsing, either to mere literary brilliancy or to aridity in theories of ultimate reality, or in the massing of experimental data on points not always selected with breadth, wisdom and perspective. It is doing a work for the child at school akin to that of the Reformation for the religious life of the adult, and the verdicts on many of the most important questions of method and matter for all educational grades, from birth to college, when fully rendered will be more or less final and will give education what it has long lacked—a truly scientific basis, and help to give to teachers a really professional status.

A. LIST OF TOPICAL SYLLABI IN ORDER.

1. Anger, G. S. Hall, Oct., 1894.
 2. Dolls, G. S. Hall, Nov., 1894.
- " (Supplementary Questionnaire.) A. C. Ellis, June, 1896.

3. Crying and laughing, G. S. Hall, Dec., 1894.
4. Toys and playthings, G. S. Hall, Dec., 1894.
5. Folk-lore among children, G. S. Hall, Jan., 1895.
6. Early forms of vocal expression, G. S. Hall, Jan., 1895.
7. The early sense of self, G. S. Hall, Jan., 1895.
8. Fears in childhood and youth, G. S. Hall, Feb., 1895.
9. Some common traits and habits, G. S. Hall, Feb., 1895.
10. Some common automatisms, nerve signs, etc., G. S. Hall, March, 1895.
11. Feeling for objects of inanimate nature, G. S. Hall, March, 1895.
12. Feelings for objects of animate nature, G. S. Hall, April, 1895.
13. Children's appetites and foods, G. S. Hall, April, 1895.
14. Affection and its opposite states in children, G. S. Hall, April, 1895.
15. Moral and religious experiences, G. S. Hall, May, 1895.
16. Peculiar and exceptional children, G. S. Hall and E. W. Bohannon, Oct., 1895.
17. Moral defects and perversions, G. S. Hall and G. E. Dawson, Oct., 1895.
18. The beginnings of reading and writing, G. S. Hall and H. T. Lukens, Oct., 1895.
19. Thoughts and feelings about old age, disease and death, G. S. Hall and C. A. Scott, Nov., 1895.
20. Moral education, G. S. Hall and N. P. Avery, Nov., 1895.
21. Studies of school reading matter, G. S. Hall and J. C. Shaw, Nov., 1895.
22. School statistics, G. S. Hall and T. R. Crosswell, Nov., 1895.
23. Early musical manifestations, G. S. Hall and Florence Marsh, Dec., 1895.
24. Fancy, imagination, reverie, G. S. Hall and E. H. Lindley, Dec., 1895.
25. Tickling, fun, wit, humor, laughing, G. S. Hall and Arthur Allin, Feb., 1896.
26. Suggestion and imitation, G. S. Hall and M. H. Small, Feb., 1896.
27. Religious experience, G. S. Hall and E. D. Starbuck, Feb., 1896.
28. A study of the character of religious growth, E. D. Starbuck.
29. Kindergarten, G. S. Hall, Anna E. Bryan and Lucy Wheelock, March, 1896.
30. Habits, instincts, etc., in animals, G. S. Hall and R. R. Gurley, March, 1896.
31. Number and mathematics, G. S. Hall and D. E. Phillips, April, 1896.
32. The only child in a family, G. S. Hall and E. W. Bohannon, March, 1896.
33. Degrees of certainty and conviction in children, G. S. Hall and M. H. Small, Oct., 1896.
34. Sabbath and worship in general, G. S. Hall and J. P. Hylan, Oct., 1896.
35. Questions for the study of the essential features of public worship, J. P. Hylan.
36. Migrations, tramps, truancy, running away, etc., vs. love of home, G. S. Hall and L. W. Kline, Oct., 1896.
37. Adolescence and its phenomena in body and mind, G. S. Hall and E. G. Lancaster, Nov., 1896.
38. Examinations and recitations, G. S. Hall and J. C. Shaw, Nov., 1896.
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A COMPRESSED AIR DEVICE FOR ACOUSTIC AND GENERAL LABORATORY WORK.¹

By GUY MONTROSE WHIPPLE, Ph. D.

A short time ago, I published a brief description of a simple form of compressed air apparatus which served a useful purpose in experiments with the Appunn tonometer and the Stern blown bottle.² At the suggestion of Dr. Stern, I have since elaborated this air-compressor until it is now in a form which renders it useful for many kinds of acoustic work, such as the actuating of bottles, organ-pipes, reed-boxes, Quincke tubes, the Galton whistle and like instruments. It is equally serviceable for any other laboratory purposes which require a perfectly uniform blast of air at moderate pressures and without the presence of hisses or other disturbing noises. The new form will also satisfy a requirement which is not met in the bellows type of machine, for it allows entire freedom to the operator; aside from the brief time consumed in changing the counterweight every two or three minutes, a continuous blast of air at constant pressure is automatically provided.

The original cruder form of the air-compressor was built upon the principle of the gasometer. A galvanized iron cylinder, 30 cm. in diameter and 75 cm. long, was inverted and lowered into a second slightly larger cylinder filled with water. Suitable weights caused the upper cylinder to exert a pressure upon the air above the water, the upper sliding down within the lower cylinder as the air was used. The upper cylinder was then raised with a counterweight when exhausted.

While this simple form of apparatus will answer sufficiently well for many purposes, it has certain defects which become serious when it is desired to work more rapidly and with greater exactness. In the first place, the supply of air is exhausted in from 15 to 30 seconds. Secondly, it can be renewed only by allowing the rising cylinder to draw in a fresh volume of air, a process which takes some three-quarters of a minute. And, finally, the displacement of water by the descending cylinder gradually decreases its effective weight and therewith the air pressure.

In the improved form of apparatus, the first defect has been

¹From the Laboratory of the Department of Education, Cornell University.

²This *Journal*, XII, 1902, 221.

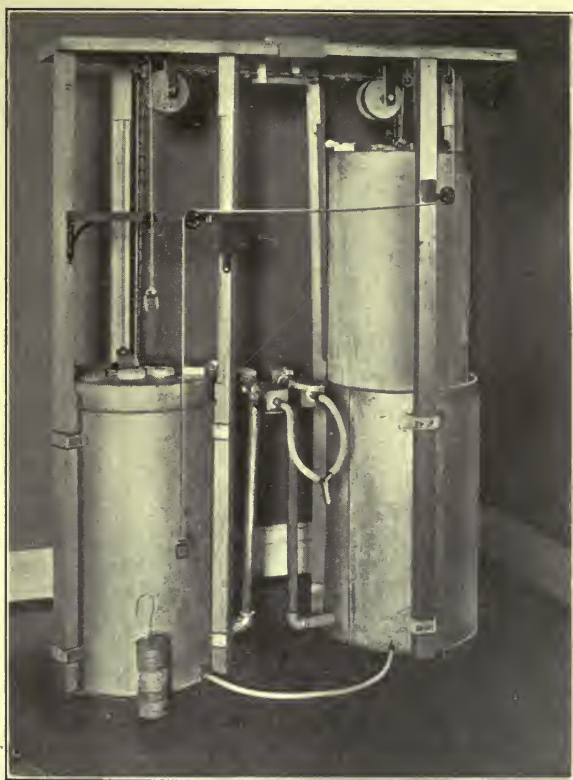
overcome by increasing the size of the tanks. The second defect has been overcome by employing two tank systems, which are exact duplicates of each other, and so arranged that the one tank is filling while the other is emptying. The third defect has been removed by a compensating device, which serves at the same time to join the two moving cylinders.

The general appearance and mode of construction will be understood from the accompanying cut, which represents the somewhat rough 'home-made' product of our experimentation. It will be seen that in each tank system there is a water tank 82 cm. high and 48 cm. in diameter. This tank is in reality double walled, as there is an inner cylinder 43 cm. in diameter, invisible in the cut. These cylinders have, of course, a common bottom, so that the space between them is watertight. This dispenses with a great deal of weight which would otherwise result if the entire tank were filled with water. The outer wall is pierced near the bottom to admit a half-inch pet-cock, which facilitates the draining of the water when necessary, and which also serves to connect the water spaces of the two tank systems, as is desirable under some conditions. Both the outer and the inner walls are also pierced, as near the bottom as possible, to admit a 1-inch galvanized iron pipe for the air transmission. Six lugs or handles for the attachment of vertical supports (iron rods or wooden posts, 181 cm. long¹), are riveted and soldered to the outer cylinder, three near the top, three near the bottom. A solid wooden plank caps the posts and forms a support for raising the tanks.

The upper or moving tanks are 79.5 cm. long and 45.5 cm. in diameter, so that, when inverted, they slide easily into the water space of the lower tanks. On the center of the flat top a heavy ring or iron loop is fastened to serve as the attachment of the connecting chain and counterweight cord, and at three points on the edge are fastened the pulleys (3.5 cm. in diameter) which serve to guide the cylinders by travelling along suitable ways on the vertical supports.

The two tank systems thus constructed are now placed side by side with the air pipes facing each other. The wooden top-boards are joined by two small braces. The moving tanks are connected by an iron chain (180 cm. long and *circa* 2.6 kg. in weight) such as is sold for agricultural machinery as "Number 052 link belting." This passes over two large wooden pulleys (13 cm., outside diameter) which are firmly fastened to the wooden top-board of each tank system. A simple support must also be placed midway between the pulleys:

¹In the apparatus pictured, wooden joists, 5.5 x 4.5 cm. cross section, were used. Iron pipes would be quite as efficient and much lighter in appearance, as only two uprights would then be required for each tank.



otherwise vibration of the chain will produce a 'puffing' effect upon the air blast. This chain is employed instead of a more flexible material, such as rope or leather belting because it possesses just the required weight. Evidently, in using the duplicate tank form of compressor, the error of displacement is doubled, for when one tank descends and thus loses weight by immersion, the other tank rises at an equal rate and thus gains in weight. Since the rising tank, from the manner of construction, is dragging on the descending tank, its variable weight-error is added to that of the descending tank. The chain used weighs per cm. exactly twice the amount lost per cm. by one tank as a result of displacement; in operation it therefore exactly counteracts the displacement-error.¹

Pressure is exerted upon the air system by means of a counterweight (*circa* 9 kg.) which nearly removes the pull of one tank upon the connecting chain, thus allowing the other to drop by its own weight. For some purposes the weight of the air tank itself furnishes sufficient pressure, but in many cases it is necessary to increase this pressure. This is most easily done, as shown in the illustration, by placing flat lead weights upon the top of the tank. It is obvious that weights of equal value must be placed upon both tanks and also upon the counterweight.

To facilitate the rapid handling of the apparatus a single counterweight only is employed. By means of small pulleys a cord is brought from the top of each air tank to a point in front of one of the tanks and about 136 cm. from the floor. After passing over the last pulley each cord terminates in an iron ring; this serves by its weight to keep the cord sufficiently taut and furnishes an easy means for attaching and detaching the counterweight. The latter is made preferably of lead with removable discs to vary its weight. It is clear that, when one tank is emptied, the other can be at once set in motion by simply lifting the counterweight from the lower to the upper cord-ring, an operation which consumes less than five seconds. *This brief task of occasionally raising the counterweight is thus the only attention which the air-compressor demands: the operator is otherwise entirely free to conduct his experiments.*

The air system, as has been mentioned, is made up in part of a short (11 cm.) iron pipe projecting from the bottom of each water tank. This piece is continued upward by an elbow and straight piece (76 cm. long) which terminates in a T-piece with a short pipe projecting from each arm. Upon these arms are screwed the check-valves, two for each tank system (one

¹ This might be effected, perhaps, by the use of a flat, flexible belt of thin leather or woven linen, to which small lead weights should be fastened at frequent intervals. The pulleys could then be made much smaller.

opening inward and the other, which leads to the instruments, outward). It will be seen that these valves are essential to the rapid manipulation of the machine, for otherwise it would be necessary to turn four valves whenever either tank was exhausted. As it was impossible to purchase air-tight valves of sufficient delicacy, these were constructed in the laboratory. Though they are somewhat bungling in appearance they are easily and cheaply made and work to perfection. Each valve is built in a wooden box $7.5 \times 7.5 \times 11$ cm. The joints are made tight with melted paraffine, though the top is preferably screwed down against a layer of chamois or thin rubber smeared with white lead, so as to be more easily removable for inspection or repair. The ends of the valve boxes consist of wooden blocks bored to receive the iron pipes. In each box one of these pieces is planed off on its inner surface at an angle of about 15 degrees (the longer face being on the bottom). A piece of brass tubing (30 cm. in diameter) is then driven into the block and brought to an even edge slightly projecting from the oblique surface. This forms the seat of the valve. The flap is made of a sheet of thin flexible rubber (such as is sold in strips for bandages) to which is cemented a piece of 1-16th-inch hard rubber slightly larger than the seat of the valve. The soft thin rubber, being next the brass tubing, forms an air-tight valve of the 'flap' or 'butterfly' type (the upper end serving at the same time as the hinge), while the hard rubber provides a perfectly flat, rigid surface at the line of contact. The outward opening valve-boxes have attached to them a half-inch pipe leading to the instrument to be blown.

The amount of air available in the tanks just described (over 128 liters before compression) is sufficient to actuate a small organ pipe (*e. g.* of 220 vib.) continuously for two minutes 45 seconds and the Stern blown bottles nearly as long, *i. e.*, about eight times as long as the smaller machine first constructed. In case a large quantity of air at a relatively high pressure is desired for shorter periods, this may be readily secured by raising both tanks and allowing them to fall simultaneously.

In comparison with the large Appunn bellows,—the laboratory instrument most commonly used for like purposes,—the compressed air device which I have described will be found to present many advantages. It occupies no more floor space; it delivers air with absolute uniformity and at far less expenditure of time and energy on the part of the operator. Finally, its first cost is much less¹ and it is distinctly less liable to deteriorate with use and age.

¹ The writer will gladly submit estimates for the construction, in Ithaca, of duplicates of this apparatus.

PROFESSOR CALKINS ON MENTAL ARRANGEMENT.¹

By I. MADISON BENTLEY.

In a recent note (*Philosophical Review*, XI, 553), Professor Calkins makes reference to my article on "the Psychology of Mental Arrangement" in the April number of this *Journal* (XII, 269). The note calls for the following explanatory remarks. (1) It is true that I did not take account of Münsterberg's *Wertqualitäten* or Ebbinghaus's *allgemeine Eigenschaften*; but neither did I discuss the views of Wundt or of Spencer or of James. My primary object in the article was to trace a line of discussion that had run through the magazine literature during the last twelve years—a discussion that was fairly coherent and logical. Since I made no attempt to give an account of the text-books and of the systematic treatises, the limitations of the article seemed obvious and I did not think it necessary to point them out. I referred to Stout because he had proposed an English term to cover the alleged formal elements; a term that has had some influence on English terminology (see, *e. g.*, Baldwin's *Dict. of Philos. and Psych.*). (2) If I sounded a warning against the hasty recognition of 'form qualities' and 'funded contents,' I should like to make the warning twice as emphatic against the illegitimate use of relations. Professor Calkins's argument for 'relational elements' is that the expression "suggests that 'dependent' character of these experiences which Cornelius marks by naming them the 'attributes' of conscious complexes." But if it is the attributive characteristic that is peculiar to the relational element, one can but wonder why Professor Calkins has made the usual attributes of sensation independent elements, and has created separate classes for 'attributive' and 'relational' elements, (see her *Introduction to Psychology* pages 105 and 113). Moreover, dependence is characteristic—both in the physical and the mental world—of other things than relations. A relation is a single type of dependence. The height of the mercury column depends upon temperature, feeling depends upon intensity of sensation, æsthetical appreciation upon a complex of processes, but no one of these dependent terms can properly be called a relation. It might, indeed, be said that, were every

¹This note should have appeared in the October *Journal*, but was omitted by mistake. E. B. T.

dependent element a 'relational' element, all consciousness would be relational; for consciousness depends invariably both upon nervous processes and upon previous conscious experience. "Divorced from Spencer's associationist interpretation," and from the notion that mind must contain things and relations because it knows a world of related things, the term becomes, it seems to me, void of contents. The word 'funding' is itself much more significant and much less misleading. (3) Since I did not discuss the experiences of 'oneness,' of 'likeness' and of 'difference,' and since I did not commit myself to two classes of elements—"sensational and affective phenomena,"—I can hardly be held responsible for my critic's inference that I should deny the existence of a unique factor in these experiences; though I may say at once, that I should gravely question the propriety of naming such a factor when found a relational element. I may add that I did not go so far, either, as to assert that some factor analogous to funded contents could, in every instance, be dispensed with. My contention was that "the hypothesis of distinctive and unique conscious structures which characterize mental complexes is to be entertained with caution if not with suspicion," and that "their intemperate use in certain of the treatises that we have discussed is both unnecessary and indefensible." There was, moreover, a plea entered for a clearer comprehension of analysis and, also, for the rejection of the atomic view of mind—a view which, I am convinced, is really at the core of the theory of relational elements. As to the type of complexes which the article did bring under discussion—the type represented by figures and melodies—there seems to be no difference of opinion. Professor Calkins says: "nor does direct introspection disclose the presence of specific form-qualities as distinctive of particular melodies or figures." (4) It is possible that Professor Calkins has overinterpreted the proposed 'pattern' or 'plan of arrangement' which was explained as "nothing but the elements taken together." It is precisely the introduction into mental synthesis of any sort of adhesive material that the 'arrangement' theory is intended to preclude. On the assumption that an element is a concrete, self-contained entity,—a common enough artifact of analysis,—the notion of such a 'plan' would be foolish; on the assumption of the type of analysis for which the article contended, the notion can hardly be termed either "vague" or "ambiguous."

LITERATURE.

The History of the Problems of Philosophy, by PAUL JANET and GABRIEL SÉAILLES. Edited by Henry Jones. Vol. I, pp. 389; Vol. II, pp. 375. Macmillan and Co., London, 1902.

The psychological volume discusses what is philosophy, the psychological problem, the senses and external perception, reason, memory, association of ideas, language, feeling, freedom and habit. The philosophical volume treats of ethics and its problems in ancient and modern times, metaphysics including scepticism and certitude, matter, mind, and their relations, and in part four, theodicy with a religious problem in ancient, middle and modern times, with a final chapter on the problem of the future life.

A Syllabus of an Introduction to Philosophy, by WALTER T. MARVIN. (Columbia University Contributions.) The Macmillan Co., New York, 1899. pp. 279. Price, \$1.25.

Conception, definition, and classification are discussed in the introduction. Under metaphysics, realism including ontology and cosmology, and then idealism are characterized. Then follow the problems of conceptual knowledge and those of the principles of reality, while the last three parts are devoted to the philosophy of religion, aesthetics and ethics.

The Light of China. The Tào Teh King of Lâu Tsze; 604-504 B. C. An accurate metrical rendering, translated directly from the Chinese text, and critically compared with the standard translations, the ancient and modern Chinese commentaries, and all accessible authorities, by I. W. Heysinger. Research Publishing Co., Philadelphia, 1903. pp. 165.

This, we are told, is the ninth translation of the text of Lâu Tsze, born 604 B. C., into the Western languages. The author has chosen a metrical form of translation, but has traced every word of the eighty-one chapters to its source. The original was in pure Chinese poetry. Beginning with the one hundred and ninth page is an index, and following that, a list of words of special significance. While we cannot judge of the fidelity of the translator, the work itself is full of interest and a Godsend to the student of literature, philosophy and religion.

Religion as a Credible Doctrine. A Study of the Fundamental Difficulty, by W. H. MALLOCK. Chapman and Hall, London, 1903. pp. 287.

The chief chapters here treat of methods, starting point, origin of life, animal immortality, five aspects of the free will problem, psychic and material determinism, religion and the God of philosophy, sentient life and ethical theism, practical basis of belief, the reasonable liberation of belief, etc.

Studies in the Apostolic Church, by CHARLES H. MORGAN, THOMAS E. TAYLOR, S. EARL TAYLOR. Jennings and Pye, Cincinnati, 1902. pp. 226.

The aim of this work is to enable those who use it to master this part of the Bible "and to impart such a knowledge of the life and

work of the early disciples of our Lord as will lead to the highest Christian character and service." It is essentially a work of synopses of Sunday School work, divided into lessons and days with question and answer.

Human Personality and its Survival of Bodily Death, by FREDERIC W. H. MYERS. Vol. I, pp. 700; Vol. II, pp. 660. Longmans, Green and Co., London, 1903.

This posthumous work has long been expected and really sums up the views of its author who was perhaps the most influential member of the movement known as psychic research. The main topics treated are:—disintegrations of personality, genius, sleep, hypnotism, sensory automatism, phantasms of the dead, motor automatism, transpossession and ecstasy. The editors have given a very valuable digest of the contents of each volume, but only careful reading can do justice to the great industry and ingenuity of this subtle and well trained mind. The contributions, which he has made in this book and previously, the conceptions of the relations between the sub-conscious and the normal mind, will always give him a high rank among psychologists. No one has contributed more toward the clearing up of portions of that vast field that lie between normal common sense and insanity. He has enriched many portions of this field by very valuable new facts collected from a very wide area and has given very many subtle explanations and made suggestions right and left of the highest value. Fortunately this work can now be tolerably well demarcated from his own pet hobby of objective demonstrations of post-mortem spiritual existence. No man was ever more supremely dominated by the desire to demonstrate immortality. This was the passion of his life. It largely determines the selection of his facts and colors every description of them. But, fully persuaded as we are that all this is as mistaken as it would be to interpret the facts of astronomy back to the formulæ of astrology and utterly inconclusive as it all is, we believe he has opened a new and rich mine for other theories which perhaps may ultimately arise and be the exact converse of his. It must here suffice to add only that to our thinking the key to the explanation of every phenomena is to be found in the past and not in the future, and when the great work of developing the doctrine of psychic evolution is complete many of his own facts will shine with a new lustre and point perhaps toward an utterly different goal and one which he would perhaps abhor.

L'Hypnotisme et la Suggestion, par DR. GRASSET. O. Doin, Paris, 1903. pp. 534.

The author of this attempt at a psychological synthesis is inspired by Pierre Janet and holds to his distinction between a superior and inferior psychism. He agrees with Bernheim that hypnotism is a state of suggestibility, but differs from this author in distinguishing between suggestion and persuasion, advice and education. Its curative effect he prefers to call pediatric rather than pedagogic. It is provided with good indexes and summaries.

Modern Spiritualism. A History and a Criticism. By FRANK PODMORE. Vol. I, pp. 307; Vol. II, pp. 374. Methuen and Co., London, 1902.

The author acknowledges his great indebtedness to Mrs. Sidgwick and Drs. Hodgson and Myers. This work gives an excellent history of the pedigree of spiritualism in Book I from the early times, including Paracelsus, Mesmer, Bertrand, Esdaile, and the American Movement, especially Andrew Jackson Davis. Book II is devoted to early American spiritualism beginning with Arcadia, and describing

the physical phenomena of clairvoyance, trance speaking, etc. Book III treats of spiritualism in England from the days of Elliotson in the Zoist and Robert Dale Owen down to the present time. Book IV treats the problems of mediumship, such as slate writing and automatism, with special chapters on a Dunglas home, Stainton Moses and Mrs. Piper. On the whole it is work of great value, and the story is faithfully told.

The Mystery of Sleep, by JOHN BIGELOW. Harper and Bros., New York, 1903. pp. 216.

This is an interesting general treatise, which does not attempt to go deeply into the modern psychology of the subject, but moves in the field of early authors, religion, health, etc. It is a convenient work to have at hand, because of its quotations and summaries of earlier views, but cannot be said to add much to our scientific knowledge.

Soul Shapes. T. Fisher Unwin, London, 1890. pp. 53.

This anonymous pamphlet was suggested by Francis Galton's work in visualization, which showed that some people conceived days of the week and numbers as colored, round, speckled, etc. This author claims to visualize souls and pictures in color four types of soul. Two are deep and two are superficial. The surface soul is most complex; the map of it shows the various faculties. The deep soul is much smaller and dark brown, with only a few red patches. The mixed soul is an oblate spheroid, yellow at the surface and darkening into brown at the center; while the blue soul, the highest and simplest type of all, is cerulean. The white soul, which is not painted, is God. Escaped souls try to seize others; they have all fallen away from God.

Zur Frage der Dementia præcox, von MAX JAHRMÄRKER. C. Marhold, Halle, 1903. pp. 119.

The strong point of this interesting pamphlet is the cases, large numbers of which have passed through the author's clinic. His main point is that Kraepelin's interpretation is too negative and does not recognize the great variety of ways and symptom-groups by which dementia is attained.

Introduction à la médecine de l'esprit, par MAURICE DE FLEURY. F. Alcan, Paris, 1900. pp. 477.

The chief topics treated are the education of Salpêtrière, doctors and justice, doctors and literature, doctors and psychology, fatigue; and under moral medicine, laziness, depression, anger, and their treatment, medicine of the passions, with a concluding chapter on modern morals.

Biographic Clinics. The Origin of the Ill-Health of De Quincey, Carlyle, Darwin, Huxley and Browning, by GEORGE M. GOULD. P. Blakiston's Son and Co., Philadelphia, 1903. pp. 223.

Here is something certainly new. The author carefully collates the facts quoted from biographies of these five characters and then sums up a critical estimate of the health of each man, evaluating the effect of the different forms of the handicap by disease.

The Mental Status of Czolgosz the Assassin of President McKinley, by WALTER CHANNING. From the American Journal of Insanity, 1902, Vol. LIX, No. 2.

This is a very valuable work and the best complete summary of the whole matter, by the man perhaps most competent to treat it. Dr. Channing's conclusion is that insanity is the most reasonable and logical explanation of the crime.

The Use of Words in Reasoning, by ALFRED SIDGWICK. A. and C. Black, London, 1901. pp. 370.

The first part of the book treats the nature of reasoning under the heads—aim and method of logical study, reasoning and syllogism, reasoning and generalization, reasoning and judgment. The second part, description and ambiguity, discusses the nature of classes, indefiniteness and the progress of knowledge. The third part is on the leading technicalities of formal logic, kinds of name or term, kinds of assertion, argument and reasoning. The last part sums up the case against formal logic and suggests how it should be taught.

The Principles of Logic, by HERBERT AUSTIN AIKINS. Henry Holt and Co., New York, 1902. pp. 489.

Professor Aikins has unusual preparation for writing such a book, which seems to us from a cursory survey to be on the whole the best text-book before the public, as indeed the last ought to be.

L'Année Psychologique, par ALFRED BINET. Vol. VIII, Schleicher Frères Paris, 1902. pp. 757.

The first 389 pages are devoted to original articles, sixteen in number; then follow the digests and discussions ending with page 583; the rest of the volume being devoted to titles.

The Psychological Review Monograph Supplements. Vol. I, Harvard Psychological Studies, edited by Hugo Münsterberg. The Macmillan Co., New York, Jan., 1903. Vol. IV, No. 1, pp. 654.

This imposing volume is made up of sixteen studies of recent years in the Harvard Laboratory. All are experimental in their origin except that of Professor Münsterberg on the position of psychology in the system of knowledge to which a very elaborate chart, mapping out the field of life, truth, theoretical and practical knowledge, etc., is added. Studies of perception lead with six articles; then comes memory with three; æsthetic processes with four; and animal psychology with two. Another volume is promised.

L'Art et la Beauté, par LOUIS PRAT. F. Alcan, Paris, 1903. pp. 285.

This work is in the form of a Platonic dialogue, in the garden of the Academy, on the philosophy of art. While the form is Greek and Kallikles is a modern sophist, the drama of opinions is essentially modern. The views of certain writers stand out very clearly and there are plenty of fables and allegories that play upon modern events, and even a female philosopher, Areta, is introduced.

Heredity and Social Progress, by SIMON N. PATTEN. The Macmillan Co., New York, 1903. pp. 214. Price, \$1.25.

This is a very brief and concise discussion of acquired characters, emotion, reproduction, responsiveness, sensation, visualization, devotion, character, education, etc. We must say that this author's psychology is something which is at some points very new and strange to our guild. Emotions, *e. g.*, are made primarily destructive; acquired characters act through association of ideas, etc.

Pure Sociology. A Treatise on the Origin and Spontaneous Development of Society, by LESTER F. WARD. The Macmillan Co., New York, 1903. pp. 607. Price, \$4.

The parts of this comprehensive work are—first, taxis, including the general characteristics, establishment, subject matter and method; second, genesis, including filiation, the dynamic agent, the biological origin of subjective faculties, the conative faculties, social mechanics, statics, dynamics, classification of social forces, ontogenetic and phylo-

genetic and sociogenetic forces; third, telesis, which discusses the biological origin of objective faculties, non-advantageous faculties, the current quest of nature, and the specialization of achievement.

The Uganda Protectorate, by SIR HARRY JOHNSTON. 2 vols. pp. 1018. Hutchinson and Co., London, 1902.

This very elaborate work, with 506 illustrations, 48 full page colored plates and 9 maps, is an attempt to describe the physical geography, botany, zoology, anthropology, languages and history of the territories under British protection in East Central Africa. The book fittingly opens with a colored photograph of a new animal discovered by the author and called the okapi, which seems something of a cross between a deer and a zebra. It is difficult to over estimate the great value of a work like this, based as it is upon studies at first hand.

Tsimshian Texts, by FRANZ BOAS. Govt. Print, Washington, 1902. pp. 244.

The texts themselves are reproduced in English letters with the aid of diacritical and other points and with an interlinear literal translation in footnotes, which take about two-thirds of each page, the upper part of the page being a more continuous and coherent translation of the stories.

Nietzsche et l'Immoralisme, par ALFRED FOUILLÉE. F. Alcan, Paris, 1902. pp. 294.

The first book characterizes Nietzsche's general philosophy; the second, his individual and aristocratic immoralism; the third, Guyot's opinion of Nietzsche from his unpublished documents; the fourth, Nietzsche's religion.

Existence, Meaning and Reality in Locke's Essay and in Present Epistemology, by A. W. MOORE. (The Decennial Publications.) University of Chicago Press, Chicago, 1903. pp. 25.

Proceedings of the Society for Psychical Research. Vol. XVII, Part 45, February, 1903. London, England.

Die Neuronenlebre und ihre Anhänger, von FRANZ NISSEL. G. Fischer, Jena, 1903. pp. 478.

Ueber den Einfluss von Nebenreizen auf die Raumwahrnehmung, von HAYWOOD J. PEARCE. W. Engelmann, Leipzig, 1903. pp. 81.

Human Nature and the Social Order, by CHARLES H. COOLEY. Charles Scribner's Sons, New York, 1902. pp. 413.

Philosophische Studien. (Wilhelm Wundt.) W. Engelmann, Leipzig, 1902. Vol. XVIII, Part 3, pp. 513.

Haller Redivivus, von HUGO KRONECKER. K. J. Wyss, Bern, 1902. pp. 26.

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HABIT.¹

By B. R. ANDREWS, A. B., A. M.

A habit, from the standpoint of psychology, is a more or less fixed way of thinking, willing, or feeling acquired through previous repetition of a mental experience. A freshman goes up to college with little love for the institution. He soon assumes the conventional attitude of "loyalty to Alma Mater." The experience is repeated time and again through his college life, and this feeling attitude becomes habitual. The habit, strictly speaking, is the similar form as regards feeling which consciousness repeatedly takes; it is the fixed way in which the stream of mind flows when these familiar feeling processes form its content. It is not the familiar feeling consciousness itself; it is simply the proceeding of mind in such a way that the familiar processes are in consciousness.

The physician acquires a volitional habit of taking the pulse and asking patients certain questions. The habit is the familiar way in which his consciousness runs its course during a diagnosis. It is not the familiar consciousness corresponding to the diagnosis. It is the familiar shaping which consciousness takes, as distinguished from the processes which make it up. The conscious habitual content is not unimportant in a study of habit, as we shall see presently; but habit in itself lies outside consciousness. It implies simply an accustomed way of reacting.

Again, the lawyer has a habit of thinking in terms of law. If a problem is presented, he thinks at once of its legal aspects, and his conclusions are reached by the logic of law. In this

¹ From the Psychological Seminary of Cornell University.

habit of thought, as in the habits of volition and of feeling just cited, habit is a particular shaping which consciousness shows. It is not a conscious process or processes. To express in a word that habit means the accustomed way of feeling, willing, or doing; the more or less fixed course of consciousness with repeated experiences; the shaping of the familiar consciousness rather than that consciousness itself; one may say that habit is the mode of mental functioning when repeated processes are in mind. Just as memory is, strictly, the way in which the mind reacts when we are remembering, and not the conscious content remembered, and just as imagination is the peculiar shaping of consciousness when we build air castles and create poetic thought, so habit is the way consciousness runs its course when familiar processes are experienced. We may define habit, therefore, as that mode of mental functioning in which repeated processes are predominant in consciousness. Psychology commonly considers volition, feeling, and intellection, the three typical functions of mind. Every mental experience may be subsumed under one of these three categories. Alongside this tripartite classification, there may be placed another division, habitual and non-habitual functioning. Under the former are included all repeated mental experiences; under the latter, all novel experiences. It may be reiterated that these functions are not themselves conscious processes; they are simply types of consciousness, forms of reacting, which find expression in the arrangement or distribution of the processes of consciousness. Every mental process may be classed therefore as the expression of a function of volition, intellection, or feeling; and, at the same time, of habitual or non-habitual functioning. This view of habit as the mental function, or general type of mentation, under which are to be included all repeated conscious experiences, will serve to give us our bearings as we consider some of the literature on habit.

LITERATURE ON HABIT.

To review, even in a cursory way, all that has been written on habit, lies outside the limits of this paper. We can only indicate here its place in modern psychology. In preface, it may be noted that two distinct general standpoints are coming to be recognized in psychology. There is on the one hand a psychology of structure which studies consciousness as a structure, abstracting from its relations to the individual and his environment, resolves it into structural elements, sensations and affections, and aims to discover their complexes and interconnections; there is on the other hand, a psychology of function which regards conscious processes as functions of the psychophysical organism, just as digestion is a function, with

objective reference in the environment, and with meaning to the organism. This distinction will be illuminating in our study of habit.

James' discussion of habit seems on the whole the most adequate.¹ Yet it is exclusively a functional treatment of the problem. The basis of habit, he says, is physiological. Consciousness has for its substrate the higher nervous centers which "grow to the modes in which they have been exercised." When a mental process passes through the mind, a characteristic nervous excitation occurs in the brain. The latter in its passage through the nervous tissue leaves a trace which facilitates the repetition of the same nervous process. Hence, the corresponding mental process is likely to reappear. With further repetition, the path in the nervous centers deepens, and its mental correlate is experienced with corresponding greater ease. As to the results of habit, he notes, (1) that with actions, it "simplifies the movements required to achieve a given result, makes them more accurate, and diminishes fatigue;" (2) that, on the side of consciousness, it "diminishes the conscious attention with which our acts are performed." The value of habit to the psychophysical organism is emphasized, in that it makes automatic and subject to routine the greater part of the actions, attitudes, and mental processes of daily life. Three conditions favoring the development of habit are stated: (1) "We must launch ourselves with as strong and decided an initiative as possible." (2) "Never suffer an exception to occur till the new habit is securely rooted in your life." (3) "Seize every opportunity to act on every resolution you make, and on every emotional prompting you may experience in the direction of habits you aspire to gain."

In this systematic treatment of habit, James throws the entire emphasis on the functional side of the problem. Habit is discussed in its meaning to the individual, as it manifests itself in the routine of daily life, in such accustomed movements as walking and dancing, and in the habitual ways of thinking and reacting which give expression to character. All this is mind considered, not as structure, but with reference to the individual and his environment. James does show that habit means diminished attention, and that habituated complex movements run their course as a chain of processes in which the

¹James: *Principles of Psychology*, 1890, I, 104-127. The treatment of habit given by James might be criticised from the standpoint of psychological exposition, for its emphasis upon the "ethical implications of habit," a characteristic due doubtless to its original publication as a magazine article for popular reading. This very characteristic, however, makes James' chapter on habit, the more valuable for certain readers, *e. g.*, the teacher.

perception arising from each link acts, without conscious direction, as a cue and stimulus to the next. These last facts might be given a structural interpretation, but James' point of view is functional, his exposition is of the meaning of these facts to the psychophysical organism. There remains unsolved by James' statement, the structural problem: what does habit mean for consciousness; does consciousness show one pattern when functioning habitually and another when functioning non-habitually?

The accounts of habit by Sully and Stout also merit attention. Sully says, all "repeated or recurring processes of thought and action become more perfectly organized, and as a consequence, more rapid and unconscious or automatic. This result is expressed by the term habit."¹ The basis for habit, as with James, is physiological. He says that "the characteristic note of habit is mechanicality;" and that "the volitional process in its complete, fully conscious form is restricted to new, or, at least infrequent actions." Sully gives as criteria of the oncoming of habit: (1) Repetition tends to remove all sense of effort, and to render the movement easy, through muscular and nervous adjustment; (2) there is a consolidation of the processes of association involved.² He names four criteria indicative of the degree of co-ordination of a habit and suggests that the strength of habits may be evaluated by them.³ The criteria are: (1) Lapse of psychical initiation for habit; (2) Specialty or precision of response; (3) Uniformity or unfailingness of response; (4) Difficulty of modifying habitual reactions. These four characteristics manifest themselves in habit, but it would be a difficult task to measure habits by them. Sully contents himself with saying that with these criteria, the highest place in the scale of habitual co-ordinations is that of "the secondarily automatic type of movement, as when one takes out his latch-key at a wrong door. From this downward, there is a series of manifestations of habit with less and less of the characteristics just dwelt upon." This solution of the problem of a classification of habits seems hardly satisfactory, and we shall recur to it later.

Sully gives three main conditions which determine the development of habit:⁴ (1) The amount of time and attention given to the movement to make it our own. (2) The frequency with which the particular stimulus has been followed by the particular movement. (3) The unbroken uniformity of past responses. Whether this statement of conditions is adequate,

¹ Sully: *Human Mind*, New York, 1892, I, 56.

² *Ibid.*, II, 225-6.

³ *Ibid.*, II, 228.

⁴ *Ibid.*, II, 230.

may be left open here; at any rate, since habit is given a physiological basis, these conditions should be given a physiological explanation. We shall return to this point later. Sully mentions another feature of habit which we can, however, dismiss as falling outside the scope of this paper, namely, the bearing of habit on mental development.¹ Baldwin has made habit one of the two primary laws of the growth of mind.² The organism is experiencing repeated processes, and novel processes. Repeated processes are organized in the nervous substrate of mind and come to be functioned without the direction of consciousness. This acquired facility with repeated processes persists as fixed mental capital. Mind can then proceed to accommodate itself consciously to new adjustments, which in their turn are learned and passed over to habit. So the psychophysical organism makes mental progress. This relation of habit to mental development is of course a matter of genetic psychology, and is to be omitted here as our standpoint is that of general psychology.

Stout gives four characteristics of fixed habits:³ 1. Uniformity. 2. Facility. 3. Propensity: "we are prone to do what we are used to do." The proneness has two manifestations: (a) The more fixed the habit, the slighter the cue necessary and the less liable is the reaction to disturbance. (b) When customary action is interrupted or repressed, the propensity becomes conscious desire. 4. Habitual action is independent of attention.

Stout's statement of the conditions governing the development of habit, notes two factors:⁴ (1) "The tendency of any mental process to repeat itself—a tendency which grows stronger, the more frequently the process recurs." (2) The teleological disappearance of conation and attention as the habit is learned, *i. e.*, as the mental processes pass from the attentive to the automatic form. Stout finds a physiological explanation for this second factor in "the tendency of neural systems to a state of virtual stability." A mental reaction involves a disturbance of nervous equilibrium, and conative processes, as in the first steps of learning a habit, represent the regaining of equilibrium. The final learning of an habitual action, so that it is functioned mechanically, means the acquiring of a new center of nervous equilibrium appropriate to the disturbance made by the repeated action. The action now no longer disturbs nervous equilibrium sufficiently to give a basis

¹ *Ibid.*, I, 201.

² J. M. Baldwin: *Mental Development in the Child and Race*, 1895, p. 214.

³ G. F. Stout: *Analytic Psychology*, London, 1896. I, 258.

⁴ *Ibid.*, pp. 263 ff.

for conative processes. We shall recur later to the problem of a physiological explanation of habit.

The psychologists so far quoted, James, Sully, and Stout, approach mind from the functional standpoint. Let us hear a structural psychologist as well. Kuelpe, in his "Outlines," gives no systematic treatment of habit. His general problem is:¹ (1) to isolate and classify the mental structural elements, the various sensations and affections; (2) to classify complex processes, as made up of interconnected elements; (3) to investigate the state of consciousness as presenting general differences independent of the contents of experience. With such a programme, he introduces habit solely as one of several factors conditioning sensitivity and sensible discrimination, and hence, to be taken into account in their introspection and measurement. Habituation he defines as a "predisposition of consciousness," or "a tendency taking shape in a series of similar observations to experience and describe perceptions of similar character. . . . In such a series we are apt to find a certain direction and degree of the attention growing habitual and a particular category of judgment becoming preferred."² An illustration of what habit means for his system, is given in his inclusion of habituation among the general conditions of tonal fusion;³ he states that it "lends an added distinctness to the total impression, or the individual constituents of a connection, according as one or the other has been the object of repeated judgment or perception." In another reference to habituation, it is given as one of the determining conditions of feeling, since "under its influence both pleasantness and unpleasantness approach indifference."⁴ Kuelpe's only concern with habit is its influence on the structural analysis of mind into elements and their complexes. Mental processes which are repeated a few times come to show slight differences from the form in which they are first experienced. For exact psychophysical determinations, it is necessary to evaluate, or, at least, take into account, such differences. That is all habit means to Kuelpe's system. Were his exposition of the state of consciousness more complete, one might expect further structural reference to habit in an account of differences between the habitual and non-habitual states of consciousness. Neither Kuelpe nor others give such a statement, however. We shall return to it immediately as an important problem regarding habit.

¹O. Kuelpe: *Outlines of Psychology*, Trans. by E. B. Titchener, New York, 1895, p. 19.

²*Ibid.*, pp. 39, 41.

³*Ibid.*, p. 303.

⁴*Ibid.*, p. 261.

There are other writers who might profitably be examined. Enough have been cited, however, to furnish the broad outlines of the subject. Summarizing, it is evident that the functional psychologists have given the subject most attention. The quotations cited have set forth the basis of habit, the conditions of its development, its characteristics, and its meaning for the individual. Kuelpe, a typical structural psychologist, considers habit only as helping or hindering in the analysis of mind into structural elements and complexes. In this examination of the literature of habit, various problems have presented themselves. Of these, we will consider the following three: I. The differences between the habitual and the non-habitual states of consciousness: (A) as regards pattern or form of arrangement of parts; (B) as regards specific processes present in the one and not in the other. II. A classification of habits. III. The development of habits.

THE STATE OF CONSCIOUSNESS WITH HABITUAL FUNCTIONING.

Habit, as we have seen, is a particular mode of mental functioning, antithetical to non-habit. Every mental process has either been in consciousness before, or is entirely novel. Processes of the first sort are included under habitual functioning; of the second, under non-habitual functioning. The present enquiry asks what are the differences between the states of consciousness with these two kinds of functioning. The enquiry breaks into two parts.

A. The Difference in Pattern or in Arrangement of Parts.

(1) The first distinction to be included under this head is a quantitative one: the conscious processes of habitual functioning are few and meager as compared with those of non-habitual functioning. Non-habitual processes bulk large in consciousness; habitual processes bulk small. Compare for example the mental processes with the familiar act of writing one's signature and those with the non-habitual act of writing words backward. Examining the former consciousness, one finds the conscious impulse which started the signature writing and the mass of tactual, movement and visual perceptions set up by its movements. There is no series of conscious impulses initiating the part movements of the writing, or any conscious direction of them. One may assume, with James, that each part movement is set off, without volition, by the vague, unattended-to perception arising from the preceding part movement. Let us examine these two states of consciousness more in detail with a view to quantitative differences.

One may examine the habitual consciousness with signature

writing in three ways. (a) By attending to the writing in a general way, it becomes the most prominent part of consciousness. The processes prominent, however, are not the impulses directing the part movements, or the perceptions arising from the part movements. That is, the processes of the habitual functioning itself do not stand out in consciousness. The prominent processes are the mass of resulting perceptions, more or less differentiated into parts, or some suggested process, as a thought regarding the purpose of the writing, the peculiar shape of the letters, or a memory of writing the signature in starting a bank account. The processes which actually direct the part movements are meager and loom small in consciousness. (b) If attention be concentrated closely on the conscious direction of the part movements in the habitual writing, and all other processes be inhibited, in an attempt to make these habitual processes large in consciousness, one finds the habitual functioning changed into non-habitual. There are at once checkings to the smooth, mechanical course of the habit, and the writing is marked with effort. It is impossible to bring the mental processes of habit into prominence in consciousness, and maintain the habitual form of functioning. (c) If attention is directed in neither of these ways, the sparse processes which direct the habitual part-movements drop out, and other processes quite unrelated to the signature fill the field of consciousness. So, while writing, I spoke to a friend who came into the room, listened to the wind outside, and experienced other extraneous mental processes. The facts, that on the one hand, the familiar writing goes on uninterrupted by related and unrelated simultaneous processes, and, on the other, that it is impossible to make the direction of habitual movements prominent in consciousness without destroying the habitual form of functioning, indicate the small bulk of the conscious processes in habitual functioning.

In non-habitual functioning, on the contrary, as in writing words backward, the mental processes accompanying the act stand out prominently. There is a tangle of processes including the selecting of letters to be written, the rejecting of others, and visual and auditory images of the words whose letter-order is being reversed. The enumeration of these processes indicates their bulk. They fill consciousness completely, excluding other processes unless these be habitual. So, for example, the tangle of non-habitual processes involved in choosing letters in an unusual order, is accompanied by the slightly conscious habitual processes of writing the letters when chosen. Additional non-habitual functioning, however, is excluded. While writing words backward, some one spoke to me and the unfamiliar movements stopped until the interruption was past.

With signature writing, however, the habitual processes ran their course undisturbed by the presence of other processes, as of speech, audition, and memory. This fact of itself, indicates the relative meagerness of the mental processes with habitual functioning, and the fullness of processes with non-habitual functioning.

It is a fair question how "full" the non-habitual process, and how "meager" the habitual process may become. The former may occupy practically the entire field of consciousness as in the scientist's absorbed thought on some new problem, or in an instant of complete concentration with the beginner in dancing. On the other hand, the habitual processes may move temporarily outside the field of consciousness, as when while walking one becomes entirely "wrapped up" in some problem, and yet continues to walk, though unconscious of the movements. In such a case, the perceptions of part movements which serve each as a cue to the succeeding part movement, become so vague that they may be regarded as subliminal excitations. This again indicates the meagerness of the conscious processes in habitual functioning.

Concluding this point, it seems established that mental processes with habitual functioning are few and meager compared with those of non-habitual functioning. With the former, other processes in consciousness are possible and usual; with the latter, other processes, unless habitual, are excluded.

One may think of the difference between the habitual and non-habitual consciousness in terms of a cross-section of mind. Then the habitual consciousness in cross-section shows a small low wave representing the meager habitual processes, and in addition a larger wave representing the incidental processes, habitual or non-habitual, which may also be present. Were one to illustrate this graphically, one would represent the small wave in heavy lines—to indicate the determining importance of the habitual processes—and the large wave in light or dotted lines, to indicate the incidental presence of other processes. The non-habitual consciousness in cross-section shows a large, high wave representing the bulky processes of non-habitual functioning. Graphically, the large wave would be represented in heavy lines, to indicate its importance in the consciousness; the cross-section should also show, in lighter lines, a low wave to indicate habitual processes, small in bulk and in importance, which are also present, usually, in the non-habitual state.

(2) A second difference in the pattern of the habitual and non-habitual states of consciousness, arises from the fact that non-habitual functioning is consciously attended to and habitual functioning is not. This introduces into the former the

distinctions of the attentive state, in which processes attended to stand out clearly and distinctly and those attended-from are less clear and distinct. So we have in the non-habitual consciousness a pattern with two degrees of clearness: the processes of non-habitual functioning are relatively clear and distinct; and other parts of consciousness, the inhibited interfering processes and accompanying habitual processes, are vague and indistinct. The simple habitual consciousness, shows but one degree of clearness; the habitual processes are of the less distinct shading. It should be said, however, that the habitual consciousness is ordinarily not so simple in experience; but includes, in addition to vague habitual processes, others which have the character of non-habitual processes and which stand out with clearness. These differences will come out more distinctly in illustrations. In writing words backward, the non-habitual processes of selecting the letters to be written, not only bulked large in consciousness,—they were clear and distinct. Along with them ran indistinct processes; a vague consciousness of my surroundings, inhibited auditory perceptions, and dim perceptions from the familiar movements of writing. The unfamiliar processes which were attended to had one degree of clearness, they were distinct; the other processes attended-from had another degree, they were dim and obscure. With the familiar signature writing, on the other hand, the processes of the functioning showed but one degree, that of vagueness. If other processes, as those involved in perception, or thought, incidentally came into consciousness during the habitual functioning, they could be attended to, and, as a result, rise into the higher degree of clearness. The habitual processes of themselves, however, remained in the dim, somber background of consciousness.

This distinction in distribution of clearness in the habitual and non-habitual states comes out more plainly in the contrast between the mental side of an occupation like typesetting which involves an habituated routine, both mental and muscular, and one like reporting which deals with a succession of novel situations. Consciousness in the former case is of an almost exclusively habitual type; in the latter, non-habitual processes are prominent. The consciousness of the typesetter who has been doing straight newspaper composition for years, shows in general, we may believe, but one degree of clearness, the dim gray of habitual processes. The sight of the phrase in his copy and of the case before him, the feel of the stick in his hand and the line growing under his thumb, the pressure of the types as he picks them up—these form the mental side of his work and are represented in consciousness by processes of which he is normally scarcely conscious. They give his con-

sciousness a prevailing shading of the less degree of clearness. Occasionally, extraneous ideas, thoughts about the content of the copy, come into mind, or perhaps there is a moment's attentive work in adjusting the spacing of a line, and then, for the moment, processes of the brighter shading stand out against the dim gray of the habituated processes. Take his consciousness the day through, however, and it is essentially a dull monochrome, the characteristic of habitual functioning.

The consciousness accompanying the reporter's work is quite different. It is essentially non-habitual. He must be on the watch for every situation which promises news, and when such a situation is found, he must develop rapidly its every bearing and detail. This non-habitual mental functioning must be done under keen attention. Hence the mental processes involved are at the higher grade of clearness. Other parts of his work, however, the familiar questions used in interviewing, the developing of typical news-situations, are soon matters of routine, and the corresponding mental processes take on the obscure gray of habituation. So his ordinary consciousness shows the two degrees of clearness which mark non-habitual functioning, the clearness of non-habitual processes, and the obscurity of accompanying habitual processes. The typesetter's consciousness, on the other hand, is an habitual consciousness of almost a pure type: its processes show, in general, only the obscure shading of habituation.

(3) A third structural distinction between the habitual and non-habitual consciousness is in regard to the interconnection of part processes. We have noted that in complex habitual movements, like walking, it is enough to start the movements voluntarily. The chain of movements then runs out of itself. I make up my mind to go to the post office and start out. This much involves attention and is a voluntary act. I walk down the street, turn the familiar corners, and, without a thought of my movements, keep my mind busy with other matters till I find myself at my destination. The inception and conclusion of the long series of movements were conscious. But I gave no voluntary direction to the intermediate movements. So, as I write now, I form letters in a particular way, and join certain letters in a definite order to express a word which is in my mind; the word is conscious but I dash off its written symbol, involving a series of delicate, complicated muscular adjustments, and each part movement comes into its proper place smoothly and rapidly without conscious direction. The antithetical characteristic of non-habitual functioning is noticeable in learning any new adjustment, *e. g.*, learning to write in childhood. How slow and painful the child finds it! The up and down strokes and curves are drawn only under dis-

tinctly conscious direction. The pen goes wrong and must be checked by conscious inhibition; it goes right only as proper movements are voluntarily initiated and directed. At every step, right and wrong paths of movement lie open, and the child must choose consciously between them. The adult has a similar experience in learning golf, bicycling, or any set of muscular adjustments. He consciously inhibits wrong movements, and initiates right ones. In short, non-habitual processes are at loose ends, and attention must seek out the proper part movements and connect them up to secure the result desired. The penman and skillful golf player, as we have seen, have organized the part movements, so that once initiated the chain of movements runs through its course, without the hesitations and conscious choosing of paths experienced by the beginner. This characteristic of habitual functioning is to be explained in terms of association; the acquiring of a habit means the acquiring of a fixed series of associations. Each part movement has been welded to the preceding part through repeated co-existence in experience, so that the perception arising from the performance of the earlier acts as a cue to the following part. Each term calls out its succeeding term in the fixed series, and no other processes. By repetition, the complicated movement has become a series of associated links; the performance of the first evokes the second without volitional assistance, the second evokes the third, etc. There is conscious initiation of such a movement and a consciousness of its close, but its performance may become quite unconscious.

Such phrases as the "ease," "rapidity" and "smoothness" of habitual functioning, and its "automatic," "mechanical" and "instinctive" character, have their warrant structurally in the fact that habitual functioning is in terms of a series of organized associations, each term of which calls out its proper successor immediately and unconsciously.

Non-habitual functioning, as one's first effort at skating or piano playing, presents opposite characteristics; it is slow, difficult, uneven. Looked at from the standpoint of association, there is no fixed linking of step to step; each succeeding part movement must be selected consciously. The performance of one part of the movement suggests not a single associated step to follow, as in habit, but many associated steps from which the one most suitable to the end in view must be chosen. The attention, fixing on this one, brings it out clearly in consciousness while other processes are inhibited. The functioning follows the path selected. The same procedure is repeated for the next term—a mass of associated steps is suggested, some more prominently than others. Attention works over the mass, selects the most suitable one, which is followed out, and the

next link in the chain is forged. The association of steps thus joined persists and shows itself in greater facility when the series is repeated. Repetition strengthens the linking of part to part, and in time the series functions mechanically, each part suggesting only the proper succeeding part, and this without volitional direction. The non-habit has become a habit.

The relation of association to habit and non-habit throws light on the apparently anomalous fact, that it is impossible to attend to the parts of an habitual complex and consciously direct them without destroying the habitual functioning, making it non-habitual. Attention to any mental process, according to Kuelpe¹ means physiologically a "preparedness" for its neural correlate. The preparedness consists of two factors: an increased excitability of the cortical path or cells corresponding to the idea, and an opening up of the neural paths of its associated ideas. The neural processes underlying other non-associated ideas are inhibited. As a result of this preparedness, the idea attended-to comes into consciousness more clear and distinct than other ideas, and is rich in associations. Apply this to habitual functioning. Such functioning, as we have seen, is mechanical and rapid, each term unconsciously calls out its habitually succeeding process, and no other processes. Let attention now be focused on the direction of a part movement; the cortex "prepares" for its neural process. The corresponding cortical center is increased in excitability and its associated paths of functioning are opened. With habit, but one such path was opened, the one standing next in the organized series; now, many offer themselves for the successive step in functioning. The situation is exactly as in non-habitual functioning. There are many possible ways of functioning opened and a decision must be made among them. The decision is made as follows: the most "prepared" path, which is the one having among its associated ideas the end being sought, *i. e.*, the one which best leads toward that end, is thereupon attended to and the functioning follows along it. Attention to the part process opened up many possible paths; and, once opened up, a selection from them by means of attention was necessary. This arousal of possible paths and choice from them is repeated for each term as long as attention is focused on the part processes of the habit. But this is non-habitual functioning—slow, conscious, uneven. Habit becomes non-habit under attention, because attention to a part process arouses the processes associated with it, and functioning through such a tangle of processes is necessarily non-habitual.

Another fact to be considered in connection with association

¹ *Monist*, October, 1902.

is the initiation required to set off an habitual reaction. Sometimes volition is present. I decide to go walking, or to go to bed, or to eat lunch, and the habitual complex starts and thereafter runs, at least in its mechanical parts, without conscious direction. In completely mechanized habits, however, no active volitional initiation is necessary. A perception or idea quite unattended to may evoke the habitual response if it is not volitionally inhibited. I rarely go to my sleeping room save at night, and then always throw on the electric light as I enter. On two or three occasions when entering during the day and busy in thought with something else, I have unconsciously turned on the light. The perception aroused by entering the room had been habitually associated with the movement, under one set of conditions, darkness; and the reaction followed even under irregular conditions. Ordinarily it would have been inhibited, but volitional processes were focused elsewhere. There is always some stimulus to an habitual reaction, and as this illustration shows, it need not be volitional. Some perception or idea which has been in the past associated with the habitual function is sufficient, even if unattended to, and scarcely conscious. Volition, in such cases, is concerned only negatively: it must not inhibit the reaction.

It may be said, without offering details, that to the fact that habit is an organized series of associations is due some of its characteristics which Sully and Stout note: its "uniformity," "unfailingness," "specialty or precision in response," and its "propensity" in so far as this last term indicates the readiness of a habit to respond to a slight cue. We shall say more of propensity presently.

Summarizing this account of the differences in pattern in the habitual and non-habitual states of consciousness, we may say that in the habitual state the processes are (1) quantitatively meager, (2) uniformly indistinct, and (3) are interconnected as a series of associations so that each term unconsciously evokes the succeeding term; in the non-habitual state, the processes (1) bulk large in consciousness, (2) are of two grades of distinctness according as they are attended-to or attended-from, and (3) are joined together in the functional series only as a result of conscious choosing and rejecting.

B. *Distinctive Processes of the Habitual and Non-Habitual Consciousness.*

The attentive state, over and above its characteristic pattern of distinct processes at the focus and indistinct processes at the edge of consciousness, has a characteristic process, effort, which distinguishes the attentive from the non-attentive state of consciousness. Our second and present enquiry is regarding

processes which in a similar way may be peculiar to the habitual and non-habitual states.

(1) The distinctive characteristic of the habitual consciousness is its feeling-tone or mood. When a process reappears in consciousness it is accompanied with a mood of familiarity, or "at-homeness." With further repetition, as the process becomes thoroughly habituated and mechanical, the mood shades off into vague and scarcely-conscious indifference or "of-courseness." For example, I am introduced to a stranger and next day meet him on the street. His name, a repeated mental process and hence slightly habitual, flashes into mind, and the mental processes include a mood of familiarity. Let me meet him daily and the mood of familiarity accompanying his name in consciousness speedily diminishes in intensity. I soon speak his name mechanically, and as "a matter of course" and the accompanying consciousness is quite indifferent in feeling-tone. In a similar way, as one repeats a trip through a strange part of a city, the processes called out by the sight of business houses seen before and by following the turns previously made, are marked by a feeling-tone of familiarity. Further repetition of the walk reduces the intensity of the mood and it passes over into indifference. So one comes to use the rules of arithmetic, pass to and fro in accustomed surroundings, recall the familiar names of friends and go through entirely habituated bodily adjustments, and in all such habitual functioning, the mental processes are accompanied by no distinct feeling-tone. They are quite indifferent. Yet they may be regarded as contributing positively to the vague feeling-mass of "naturalness" or "of-courseness" which forms the dim background of consciousness. A hint of what the indifferent of-courseness of habit means to mind may be had when an habitual function is interrupted, and a feeling of unpleasantness due to the interruption succeeds the of-courseness. If a familiar object is removed from one's room, if one's daily routine is interrupted, or if a name which one uses familiarly suddenly goes from mind, each such interruption of habitual functioning is accompanied by an unpleasant feeling. At the moment this succeeds the mood of "of-courseness," one is conscious that the latter contributes something to the vague feeling of naturalness in the background of consciousness. Habitual functioning is therefore characterized consciously by the feeling of familiarity; and in thoroughly organized habits, by the indifferent mood, termed "of-courseness" or "naturalness." Interruptions of habitual functioning are unpleasant in feeling-tone. This gives us the distinctive process in the habitual state of consciousness.

This affective characteristic explains in part the "impulsive-

ness" or "propensity" of habit, which we may consider for a moment. Reid¹ says that habits often involve not only facility in action but "inclination or impulse toward action," and Stout, as we have noticed, includes "propensity" as a characteristic of habit. This characteristic seems explicable by two factors. (a) The first is that of association, already mentioned. Each term of the habitual series calls out its succeeding term without volition, and the series may be initiated without volition by the presence of the perception or idea which has been associated with it as its cue. This fact accounts in part for "propensity" toward habitual functioning. Such functioning is in the line of the least resistance, both as to initiation and progress. A situation is presented in which one may speak truly or falsely. If in one's past experience truth-speaking has been the next associative term in similar situations, it is easier now to speak truly than falsely. Truth-speaking is unconsciously suggested as the next term in the functioning, and this term follows without volitional direction. In a similar way, if the hour of five has been associated daily with taking a walk, the arrival of that hour to-day suggests the walk. It is easier to follow the habituated association than to form a new one. We may say, then, that the propensity toward habit is explained in part by the slight initiation it requires and by its non-volitional course, both of which are referable to the fixity of association. (b) The other factor is the affective processes involved in habit. Habitual functioning is accompanied by the mood of familiarity, or, if the habit is mechanized, by the vague mood of "naturalness" or "of-courseness." If habitual functioning is interrupted, there is a feeling-tone of unpleasantness. These affective processes both favor habitual functioning, as against non-habitual. Telling the truth contributes to the mood of naturalness. If one hesitates to speak the truth, habitual functioning is interrupted and the unpleasant feeling resulting impels to truth-speaking. False-speaking similarly breaks in on habitual functioning and is therefore unpleasant. The affective processes favor the habitual functioning, the truth-telling. Just so, with the habitual walk: following the regular sequence adds to the vague general feeling of naturalness and of-courseness; breaking the sequence, is unpleasant. Hence there is inclination to follow the habitual order. The "impulsiveness" or "propensity" of habit is to be explained, then, by the ease of functioning in the associated order, and by the influence of the affective processes concerned.

Summarizing this section, we may say that the distinctive process in the habitual consciousness is its mood of familiarity

¹ Thomas Reid : *Essays on Active Powers*, Essay 3, Chap. 3.

with partly habituated functioning, and of vague indifferent naturalness with entirely habituated functioning. This affective characteristic of habit, together with its organization as an associated series, explains its impulsiveness.

(2) The distinctive process in the non-habitual state of consciousness is the experience of directed effort. This is plainly present in learning a new muscular adjustment, as swimming, typewriting, or the playing of musical instruments. The innervation of the proper muscles, to the proper amount, and in the proper sequence, and the inhibition of false movements, are accompanied with a consciousness of effort. The effort, further, is directed; namely, toward the organization of a series of movements which will give the desired adjustment. The end is conscious and the choosing and rejecting of movements is directed toward accomplishing it. Moreover, the feeling of effort is localized in the muscles, or the tendons of the muscles, which are being adjusted—in the finger muscles in learning the piano, in the leg and arm muscles in learning to swim. There is present, too, the general set or brace of the whole body which is always a factor in effort.

The experience of effort may be traced also in non-habitual functioning of the intellectual type. If I go into a shoe store to inspect a pair of shoes, a series of habitual processes runs through mind corresponding to my questions: "Are these shoes well made? What kind of leather? What sort of a sole? Is this in the spring style? The price? etc." The questions seem to suggest themselves, without any consciousness of effort, when I am in the familiar position of shoe purchaser. I attempted, however, to think myself into the unfamiliar position of the shoe dealer and conceive what judgments he might pass upon the shoes. I could do it only with effort. The end in view was conscious and the necessary steps toward it were made volitionally. The processes included a determination of the points of interest in the shoes for the dealer, and the judgments he might make upon them. I conceived him as thinking: "The cost of these shoes to me? Do they seem to fit and please the customer? My profits if he takes them? The grade of leather in them? Their manufacturer?" I arrived at these unfamiliar mental results only with consciously directed effort. The feeling of effort was localized physiologically about the eyes, as the shoes were examined visually, and the thinking was largely done in visual terms.

Feeling experiences, too, so far as volitional processes are concerned in them, show the same characteristic of the effort factor in non-habitual functioning. The effort, be it noted, attaches itself not to the affective side of the experience but to

the perceptions or ideas present, *i. e.*, just as it is impossible to attend to the affective side of an experience, so it is impossible to make it the basis of effort processes. One has an example of non-habitual functioning in feeling whenever an æsthetic or other judgment of sentiment is made. For example, I see a famous picture for the first time and study it carefully, passing judgment upon it point by point. The mass of processes is affectively toned and the experience is conative. The next time I examine the picture, as far as judgments once made under effort are repeated, they are repeated with diminished effort or without effort. As the picture becomes familiar, the judgments once made are accepted and used without actual conative acts of judgment. In all feeling experiences, the effort which attaches to the perceptions and ideas behind the feeling, is present only in non-habitual functioning. As in other cases, effort is directed toward an end—in the illustration cited, the forming of æsthetic judgments. This applies to one's feeling attitude toward countless things in art, in ethics, in religion, and in intellectual or logical thinking. For every such attitude intelligently taken, and not accepted at second hand, there was an original process of non-habitual conative judgment, affectively toned. As the results of the judgment came to be habitually used, the conative processes dropped out. The feeling attitude is thereafter taken without effort. There are many feeling experiences, the emotions of joy, sorrow, fear, etc., and the feelings proper, warmth, thirst, weariness, etc., in which no such processes of judgment are involved. The attention is passive, and the effort experience as in all passive attention is weak. It would be difficult to differentiate habitual and non-habitual feelings of this sort, by the effort factor. There is another criterion, however, which may be applied to such feelings and indeed to all feeling experiences: non-habitual feelings are relatively intense; with repetition, both pleasantness and unpleasantness approach indifference, whether present in a feeling proper, an emotion, or a judgment of sentiment. This general statement was implicit in our treatment of the feeling-tone of habitual functioning which diminishes in intensity with repetition, passing from the mood of familiarity to that of indifference. Other examples might be cited of this general characteristic of feeling experiences: a new suit of clothes is worn with pleasurable feelings for a few days, but in a couple of weeks it is quite a matter of course and calls out no affective reaction. Unpleasant feelings likewise decrease to indifference. On one occasion I had to work for some time under unpleasant circumstances which irritated me not a little at first. In a week or so, I was disregarding them; they had become indifferent.

Summarizing this discussion, it may be said that the directed effort experience is the distinctive process in non-habitual functioning. With feelings, further, all non-habitual processes are relatively more intense: as they become habitual, they pass to indifference.

Placing together the results of this section, we note that the non-habitual consciousness is distinguished, as just stated, by its effort experience and the relative great intensity of any feeling processes present; the habitual consciousness, by its mood of familiarity for repeated and partly habituated experiences, and of indifferent-of-courseness for thoroughly habituated experiences. For the sake of summary, we may recall the differences in pattern noted in the preceding section: the processes of the habitual consciousness bulk small, are uniformly indistinct and are fused together in an associated series so that each term unconsciously calls out its proper succeeding term and only that term. The processes of the non-habitual consciousness bulk large, are of two degrees of clearness, distinct and indistinct, and are consciously joined together as terms in the functioning series by attentive selection and rejection.

A CLASSIFICATION OF HABITS.

As we stated at the outset, habit is a mode of mental functioning in which repeated processes are in mind. A habit is one such mode of functioning; and there are as many habits as repeated processes. How can we classify them? A preliminary statement of the concept of nervous tendency in its connection with habit will assist us. A nervous tendency is a particular modification of the nervous system favoring a definite sort of functioning. Two men receive the same objective stimulus, from a green field: one thinks, "how fine for a golf course," the other, "what a good pasture it would make." The two nervous systems reacted differently because of their individual peculiarities, the nervous tendencies present in each. Habit, as we shall explain in a later section, is at bottom, a physiological phenomenon. The acquiring of a habit means the development of a persisting nervous modification, a "tendency," which expresses itself in the various conscious manifestations of habit. It will be convenient to speak of an "habitual tendency," meaning the particular neural modifications underlying an habitual mode of functioning. By examining the manifestations of habitual tendencies, one comes to a first basis of classification.

A. *Specific and General Habits.*

Every mental experience possesses in some degree the possi-

bility of reappearance in consciousness. I can recall the mental experiences of the instant just passed, but for most such experiences, the possibility of recall is brief. I remember the dishes at dinner to-night, but I have no idea what was served yesterday. I recall a deep snow in December, but other weather conditions during that month are gone beyond recall. I had to derive a mathematical formula a few days ago which I had not looked at in some years; after working at it a bit, I caught a cue and the derivation almost ran through of itself. In each of these cases where a mental experience is revived, one assumes that the original experience left a trace in the nervous substrate of consciousness making possible the recall of the mental processes. Similarly in learning a muscular adjustment, as tying a new form of neck-scarf, the first performance may be carried out only with difficulty; but the mental processes leave an effect in their substrate, a tendency, which makes a repetition of the same movements easier. Behind the habitual performance of a daily routine, one assumes a set of nervous modifications, developed by earlier performance of the duties, which provide for their machine-like repetition day by day. When I have once formed an opinion on the Sistine Madonna, this opinion comes into mind at once if Raphael's painting is being discussed. I do not wait to form a new opinion; the one which has been in consciousness before, reappears. In each of these cases, a particular set of mental processes is experienced, and an habitual tendency brought about which facilitates the re-experience of the same specific processes. Such a mode of functioning may be termed a specific habit, or, since mental contents are repeated, a habit of content.

Habitual tendencies, aside from providing for the re-experiencing of specific processes, manifest themselves by shaping the course of other processes in consciousness. One day in January the weather bureau reported a fall of 34° in temperature and a wind of 30 miles an hour. The facts were unusual and have come into mind many times since. More than that, for two or three days after the cold snap I was weather-minded. I scanned the papers to watch the progress of the storm elsewhere; I found myself discussing the weather, an unusual topic for me; and I inquired from a friend just back from New York, as to the weather there on the day of our blizzard. The habitual tendency persisting from the original experience manifested itself first in the re-experiencing of the specific processes; and secondly, in a more general way, by giving a certain form to consciousness in accord with the earlier experience. The habitual tendency which underlies truth-telling is of this more general character: it does not provide for the re-experience of specific processes, but determines a general form of func-

tioning which shapes whatever processes may be in mind so that their expression is truthful. Similarly, with the habits of punning and answering letters the day they are received, mentioned by Stout. In each, the habit determines the form of consciousness; in one case punning, in the other answering the letter at once. It does not determine the specific pun or the contents of the answer made to the letter. It is evident that we have here a second form of habitual functioning which may be called a general habit, or since it determines the form of consciousness, a habit of form.

Both sorts of habit find illustration in the "professional mind." On the one hand, the mental processes corresponding to the facts of the profession, *e. g.* law, are at the instant service of consciousness, so strong are the habitual tendencies favoring their re-experience. These are specific habits. On the other hand, the continued experiencing of the mass of related processes, which form the mental side of a profession like law, develops general mental habits in accord with these experiences. The lawyer's thought shows the impress of these habits; it follows legal logic. The memory of the professional man is best for the facts of his profession; he easily masters new situations within his field while he may be incompetent outside it; his interests are those of his profession, so that, if a lawyer, he enjoys Blackstone, while a scientific monograph would bore him. These are evidently manifestations of general habits which give varying form to consciousness in different individuals. Their influence is clearly illustrated in the different reaction which various minds make to the same situation. Let three men, an artist, a farmer, and a railroad man look out over the hills about Ithaca. The first will probably be enraptured with the beauty of the scene, the second will wonder about the quality of the soil on the hill tops, and the third will remark that there must be heavy grades on the railroads entering the city. The artist has in the past experienced mental processes pertaining to the beautiful, and so has acquired a general habit of thinking in terms of the æsthetic. His present mental experiences are shaped by this general habit. Similarly, with the farmer and the railroad man. General habits are of the most far reaching significance as accounting in part at least, for differences in mental constitution, type and temperament. In part, the tendencies that determine these manifestations are doubtless hereditary; very many of them, however, are habitual tendencies built up by past mental functioning, which persist and determine the course of our mental life in the present.

From this point of view habits may be regarded as of two kinds, specific and general. The former function in mem-

ory, imagination, thought, bodily movement, etc., in which specific processes experienced in the past, reappear in consciousness. The latter function in habits of thought, attitude and volition in which novel processes of the present are shaped in accord with the experiences of the past. The bearing of the following statement from James on this distinction, is clear: "It is not simply particular lines of discharge, but general forms of discharge that are grooved out by habit in the brain." There is possible, also, another classification of habits.

B. *Levels of Habit.*

Sully, as we noted in examining his treatment of habit, attempts to measure the "degree of habitual co-ordination," or the strength of habits, by using various characteristics of habit as criteria. But he only succeeds so far as to say that the highest place in the scale is that of "the secondarily automatic type of movement," and that "from this downward, there is a series of manifestations of habit with less and less of these characteristics." We have seen that habitual tendencies manifest themselves in two ways, providing for the re-experiencing of specific mental processes and exerting a shaping influence on consciousness. We might find in the persistence of the former characteristic and in the degree of the latter, the basis for a quantitative classification of habit. Such a measure would be difficult to apply directly. Titchener, however, has suggested a division into "levels of habit"¹ in which the lines of demarcation are primarily in accord with the sources of the habituations. By combining his "levels" with a rough application of the measure just suggested, we arrive at a fairly satisfactory classification of habits.

Habits whose basis is simply recency of occurrence are least persistent, and exert the least influence on consciousness. They form the lowest level of habit. The miscellaneous everyday experiences of only ordinary interest belong here. We remember them for a longer or shorter period varying with the depth of the impression they make upon us. They may also influence one's other conscious life for some little time, as did the unusual weather conditions. There is very little of last year's daily experiences of this sort, however, which is remembered now or which influences consciousness in a way one can be aware of. In other words, the tendencies resulting from this miscellaneous, unrelated daily functioning do not persist for any length of time. Habits of this level may be called temporary habits. Every one has the experience, however, of some scene of childhood or later life which does persist, both being

¹E. B. Titchener: *A Primer of Psychology*, 1898, p. 137.

re-experienced and exerting influence on other conscious processes. Such experiences for some reason made such an initial impression, that the resulting tendency persists for years, perhaps for life. They belong rather to the second level of habit.

Habits arising from mental experiences of unusual intensity or interest, form the second level. They are of more than usual persistence and influence on consciousness. Eating chicken is not an uncommon occurrence, and one would scarcely expect it to give rise to a persisting tendency. I ate some at a restaurant a few years ago, however, which made me ill, and I not only have a vivid recollection of that particular experience, but the tendency resulting from it still occasionally affects other conscious processes, as in making me view with suspicion chicken on a bill of fare. I have a friend whose brother was killed in the Spanish war. The habitual tendency resulting from the grief and the shock of the news persists so strongly that the sight of the national colors is enough to recall the tragic death of the brother. The memory of that single experience five years ago will doubtless be subject to re-experience throughout his life; and at the same time, the tendency making possible its recall, will influence other parts of consciousness, as his attitude toward war and international arbitration. We seem justified, then, in placing in a separate class, which we call the second level of habit, all those habitual tendencies which arise from mental experiences of extraordinary intensity, and which show an unusual degree of persistence and of influence on consciousness.

A third level of habit includes those tendencies to habitual functioning which grow up about the more or less related experiences of one's profession or employment. This class of habits is characterized as arising in adult life, rather than in youth, and as being based on the habitual round of daily activities. The definite limits of the "technical memory" and its high efficiency within those limits, professional ways of thinking, the fact that the predominant interests are those of the profession,—all point to the existence of strong tendencies arising from the habitual activities of one's occupation. Some are specific habits, as those which keep ready the body of definite facts involved in the profession; others are general, as those which shape new mental experiences in the mould of the professional mind. One finds illustrations in the technical knowledge of the engineer, the habits of thought of the lawyer and the mannerisms of the doctor. Every individual is subject to habits derived from the past experiences of his position in life and his round of daily duties, which on one hand determine specifically his knowledge, manual skill, etc., and on the other, as general habits, show themselves in peculiarities of

thought, personal bearing, attitude toward others, and in countless other ways.

The fourth level of habit includes those habits resulting from breeding, education, and early training, in short, the activities and environment of childhood. They manifest themselves in differences of speech, as between a New Englander and a Southerner; in matters of personal attitude, as politeness and courtesy; in such habits as concentrated attention, carefulness, and diligence; in manner of dress and care of the person; and in all those habitual ways of reacting which are fixed before one is twenty-five, and thereafter hold one in an unrelaxing, life-long grip. These tendencies which take shape in the plastic period of childhood, are more deeply seated than the habituations previously mentioned, and exert a stronger influence on consciousness. It is hard to over-emphasize the importance of their influence on the individual. They differentiate the city boy and his country cousin; the product of the slums, and the scion of aristocracy. When these tendencies have once taken form, it is almost impossible to eradicate them—the college boy who puts on the shop-clothes of the mechanic, is still recognizable despite his disguise; he will read a paper while his companions loaf through the noon hour, and “wash-up” at night with a care for cleanliness to which they are unaccustomed—at every turn the habits formed in youth shape his present. The Indian boy taken from a wigwam and sent through Carlisle or Hampton, in many instances, after graduation goes back to the wigwam; the tendencies of his earliest years could not be overcome by later training.

The groups of habitual tendencies so far discussed are alike in that all the tendencies concerned result from mental functioning during the lifetime of the experiencing individual. As such, they are called “acquired” tendencies. In the ordinary acceptance of the term habit, it includes only these acquired ways of functioning. It is convenient, however, for certain purposes,—and it can hardly be confusing,—to treat with the foregoing levels of habit, a fifth level which shall include all those tendencies which are innate in the individual and are hence termed “natural” or “hereditary.” Every normal child is born with a tendency to talk, and with various instincts, as acquisitiveness and curiosity. In addition, there are hereditary tendencies which vary more distinctly with the individual. We say one child is naturally musical, another stupid, another sympathetic, and so on. By this we mean that over and above the shaping influences of environment, heredity determines in part at least the characteristics of the individual. And as far as the mental side of the nature is

concerned, we mean by this hereditary endowment, that there are in every individual innate tendencies, or functional ways of least resistance in the physiological substrate of mind. These hereditary leanings are the deepest seated of all our tendencies, and the most far-reaching in their influence on consciousness. The artist is born, not made, we say; and so each individual has definite predispositions given him at birth which fit him for this or that vocation, and determine to an extent which it is impossible to define exactly, his mental possibilities, temperament, and the general course and character of his after-life. It is difficult to tell just where these hereditary tendencies end and where those acquired through early training and education begin, for in a definite case it may be impossible to trace back a tendency noticed in adult life. The distinction between the two, however, is theoretically clear: acquired tendencies, habits proper, result from mental functioning in the life of the individual; hereditary tendencies are innate. The last class of tendencies manifest themselves more commonly in determining the general form of consciousness; though, in instincts, they may give rise to specific conscious processes. Acquired habitual tendencies show themselves in the one way, or the other, or in both ways.

Summarizing this section, we may say that habit manifests itself in consciousness in two ways, in the reappearance of specific processes experienced in the past, and in shaping new processes in accord with past experiences. So we have specific or content habits, and general or form habits. Further, the classification of habits according to levels, indicates their origin and roughly, at least, the degree of their persistence and influence on consciousness.

THE DEVELOPMENT OF HABIT.

The last main division of our enquiry is concerning the development of habit. In explaining what habits are and how they come into existence, we are transferred at once from a psychological to a physiological standpoint. Modern psychology holds as a primary postulate that every mental process is accompanied by a characteristic neural process in the central nervous organs. The explanation of habit is that the passage of a particular nervous excitation, or series of excitations, through these central organs leaves them disposed physiologically for a repetition of the same nervous process. If a mental process is experienced, the passage of the corresponding nervous processes through the brain leaves that organ more ready to function with the same nervous processes again,—hence the correlated mental process is likely to reappear in consciousness. The nature of the nervous excitation is still undetermined; if

we assume that it involves some sort of molecular rearrangement in the path it follows, as seems likely, there is open to us the vivid explanation of habit which James and others give. The excitation leaves a trace or path in the nervous structure, which repetitions of the excitation deepen and widen, as running water digs its channel. The path is maintained permanently by the processes of nutrition, which follow the lines of molecular rearrangement. The persisting path constitutes the tendency to the repetition of the nervous discharge which caused it, and hence to the reappearance of the correlated mental experience. At any rate, whether or not we think in terms of actual "paths" and "canals," we shall find it, at least, necessary to assume that a nervous excitation leaves some structural change which disposes the brain to the reproduction of the same process; and that this structural trace, as we have said, provides, on the mental side, the disposition toward the reappearance of experiences formerly in consciousness. The development of a habit, therefore, means the development of a physiological modification, or tendency.

Assuming this to be the nature of habit, we shall consider the conditions favorable to its development, and correlate them with physiological changes.

1. The habitual tendency favoring the reappearance of mental processes gains strength by repetition of the processes. One learns a poem by repeating its words and thinking through its thoughts again and again. One acquires a muscular adjustment by repeating the movements; and a habit of reflection is secured by repeated processes of reflection before action. The fact is universally admitted that repetition or practice is the great factor in acquiring a habit. This is exactly what a physiological explanation of habit would expect. The first passage of the nervous excitation would leave only a slight trace, perhaps; but repetition of the same excitation would wear the path deeper. The physiological explanation, therefore, furnishes precisely the conditions demanded by the fact that a habit grows strong by repetition of the conscious processes involved.

2. A mental experience of great intensity or interest results, without repetition, in a strong habitual tendency. The case of my friend who lost a brother in the war is in point here. The intense mental experience is doubtless represented on the physiological side by a nervous discharge of great intensity. This, in a single experience, would leave a trace or path of as great "depth" as one resulting from the repetition of a weaker excitation. So, a great flood in a single day tears out a path, which a smaller stream would require years to form.

3. The fact that processes attended to produce habitual

tendencies, while those not attended-to, do not, must be considered. If I give my whole mind to learning a new set of movements, the habituation is much more rapid. The things remembered are the things which were attended to when experienced. The mental processes which later affect the course of consciousness, *e. g.*, the unusual weather conditions which made me weather-minded for a day or two, are similarly the experiences which are attended to. In general, then, if one attends to a mental experience, it gains the possibility of recall and the power of influencing other processes in consciousness; that is, the mental process which is attended to produces an habitual tendency. In Titchener's phrase, "Habit means foregone attention." Neurologically, it seems most probable that attention involves an inhibition of all nervous excitations except those underlying the mental process attended-to; these last are facilitated in their passage through the cortical centers. This gives the nervous excitations underlying the mental process attended-to, a relatively great intensity, though no absolute increase in intensity be assumed; only mental processes attended to, therefore, are represented by neural processes of sufficient intensity to leave a trace in the cortex.

4. The physiological explanation covers, too, facts of mental constitution which are best explained by referring them back to heredity. If a lawyer is legally-minded, the obvious explanation is that the experiencing of great masses of legal mental processes has built up neural paths of tendencies, which determine the form his present mental processes show. The action of the new-born infant in taking food, and its display of curiosity, however, cannot be similarly explained as the result of previous functioning on its own part. The conception of physiological tendencies covers such cases, by assuming innate nervous conditions which provide for certain reactions; conditions similar in influence to those which the individual builds up by mental functioning. So, we get our conception of hereditary and acquired tendencies, which while functioning in the same way, have a different origin. The former are innate modifications of the central nervous organs; the latter are modifications brought about by functioning.

5. Another fact to be considered is the easy formation of habits during childhood. In our discussion of the levels of habit, we noted that the strongest of our acquired tendencies arise during youth. If tendencies have a physiological basis, this would be expected. Growth and the nutritive processes generally, are most active in youth; and the bodily organs are extremely plastic at that period in life. This is especially true of the nervous system. Cortical traces produced by nervous excitations would therefore be deeper in childhood than in adult

life after the nervous organs have "set." Hence childhood would be, as it is, the great period of habit-formation. The physiological explanation fits the conditions exactly.

We may conclude that the development of a habit means the development of modifications, or tendencies, in the nervous system. These neurological changes are occasioned in the central nervous organs by the excitations corresponding to the mental processes of the habit. The chief factors favoring the development of habit are repetition of the mental processes involved, attention to them, the intensity of the experience, and plasticity of the nervous system. These factors all contribute to build up the paths of discharge which are followed by the neural processes underlying the habituated mental experience. The existence of such a path, or in other terms, the presence of the habituation tendency, is the physiological basis of habitual functioning.

GENERAL SUMMARY.

This paper has attempted to consider certain phases of the great mass of mental life termed habitual. Habit was defined as the mode of mental functioning in which repeated processes are in consciousness; a habit as the functioning, or form of consciousness, involving a particular repeated experience. Habits, as "modes of functioning," or "forms of consciousness," are not in themselves conscious; a study of habit in consciousness, therefore, is a study of the habitual conscious processes in which habit expresses itself. Heretofore, habit has been treated chiefly from the functional standpoint, in its relation to the individual and with objective reference. In this paper, we have discussed three problems with regard to habit:

1. A statement of the structural differences between the habitual and non-habitual states of consciousness. There are differences in pattern, or arrangement of processes, as follows: The processes in the habitual state are meager, uniformly indistinct, and fused into an associated series; those in the non-habitual state bulk large, are of two degrees of clearness, distinct and indistinct, and are consciously joined together by attentive selection and rejection. Further, the habitual consciousness has as a distinguishing process, the mood of familiarity with partly habituated experiences, and of indifferent-of-courseness with those completely habituated. The non-habitual consciousness has as its characteristic process, the experience of directed effort. Feeling processes are relatively strong in non-habitual functioning, and pass to indifference under habituation.

2. A classification of habits. Habits are of two classes: specific habits, which provide for the re-experiencing of past

processes; and general habits which shape present processes in accord with past experiences. Further, habits may be classified according to four or five levels: (a) those depending upon *recency* of occurrence alone for their persistence; (b) those due to mental experiences of great *intensity*; (c) those arising from the professional or other habitual daily activities of adult life, and hence referable to *recency* of occurrence and *repetition*; (d) those originating in training and other influences during childhood, and therefore, due to factors of *intensity* and *repetition*; and (e), if we may pass beyond the acquired tendencies, innate tendencies which manifest themselves like habitual tendencies in their influence on consciousness, and which may be tentatively regarded as the resultant of all the factors of habituation in some way racially summated. The order of levels indicates roughly the degree of persistence and of influence on consciousness of the habits in the various groups. Instinctive habits are strongest. Habits which exist simply through recency of occurrence are, other things equal, the weakest.

3. The development of habit. Habit involves neural modifications, or tendencies, which are brought about, save such as are hereditary, by the neural excitations underlying habitual mental processes. The important conditions favoring the development of habit, are repetition, attention, intensity of the experience, and plasticity of the nervous system.

THE INFLUENCE OF ACCOMMODATION AND CONVERGENCE^u UPON THE PERCEPTION OF DEPTH.

By J. W. BAIRD, B. A., Ph. D.

TABLE OF CONTENTS.

	PAGE.
Introduction.	
I. Historical,	151
II. Experimental,	170
i. Description of Apparatus,	170
ii. Description of Method,	173
1. With abrupt change of distance,	173
2. With gradual change of distance,	174
iii. Results.	
<i>A.</i> Monocular Abrupt Series,	176
<i>B.</i> Binocular Abrupt Series,	177
<i>C.</i> Monocular Gradual Series,	177
<i>D.</i> Monocular Estimation of Absolute Distance,	183
<i>E.</i> Binocular Estimation of Absolute Distance,	183
III. Discussion of Results.	
<i>A.</i> Monocular Abrupt Series,	183
<i>B.</i> Binocular Abrupt Series,	191
<i>C.</i> Monocular Gradual Series,	191
<i>D.</i> Monocular Estimation of Absolute Distance,	194
<i>E.</i> Binocular Estimation of Absolute Distance,	194
IV. Interpretation of Results,	194
V. Theory,	197

The present study is an attempt at the settlement of a much-disputed question: the question as to what part the mechanisms of accommodation and convergence play in our visual perception of the third dimension. For many years past, psychological opinion as to this point has been sharply divided; and the experimental data hitherto available are inadequate to a final decision of the question. The evidence adduced in the following pages points unmistakably to some such theory of visual space perception as is advocated by Wundt. A theory of space perception at large must of course be confirmed along many converging lines; and the present investigation is concerned only with a single partial problem. So far as they go, however, our results speak unequivocally for the influence of movement factors upon the visual perception of depth.

The writer's attention was first directed to the general problem of visual space perception in 1899, when he was a student of psychology in the University of Wisconsin. From 1899 to 1901 he was engaged, though with frequent interruptions,

upon a review of the literature of psychological space. Experiments upon the special problem of this paper were carried out in the Cornell Psychological Laboratory during the academic year 1902-3.

The study falls naturally, therefore, into two parts; an historical survey of the course of development of psychological doctrine regarding the visual perception of the third dimension,¹ and an account of the experimental work upon which our own conclusions are based.

I. HISTORICAL.

Attempts were made as early as the fifteenth century to determine the factors contributing to the perception of depth. | ^{15th cent.}
 Da Vinci² in his *Treatise on Painting* described various devices employed by painters for the production of the illusion of rilievo and distance. It is impossible to determine who first advanced the theory that the adjustment of the ocular mechanisms, in convergence and accommodation, furnishes a criterion of distance. It is evident, however, that this theory had been advocated as early as the beginning of the seventeenth century. Aguilonius³ mentions the theory as current in his day and attempts to refute it. In the opinion of Descartes⁴ | ^{Wallin 13.}
 both the accommodation of the refractive media of the eye, and the convergence of the visual axes contribute to the perception of distance. "The perception of distance depends primarily upon the form of the eyeball, for its form when we see a near object must be slightly different from its form when we see a more distant object. And, according as we bring about in the form of the eyeball a change which is appropriate to the distance of the object seen, we also change

¹Much of the literature quoted below has important bearings upon the question of space perception in general. It has been our aim, however, to draw upon it only in so far as it has reference to our special problem. We hope that the reader will overlook what might otherwise seem to be serious omissions in our historical sketch.

²Leonardo da Vinci: 1452-1519, *Trattato della Pittura*, English translation, *Treatise on Painting*, by J. F. Rigaud. New Ed., 1877. § § 117, 121, 124, 178, 187, 191, 199, 204-211, 283-324, 340-344, 348. It is interesting to note that, although da Vinci's analysis of the perception is far from being complete and exhaustive in the light of modern science, he yet enumerates and describes practically all of the factors employed even by the modern painter. It is a fact of interest, too, that da Vinci called attention to the influence of binocular vision upon the perception of depth. He just missed stating the principle of disparity of retinal images which Wheatstone discovered more than three hundred years later. *Treatise on Painting*, § 124; Charles Wheatstone, *Philosophical Transactions*, 1838. pp. 371-394.

³Francisci Aguilonii: *Opticorum libri sex*. Antwerp, 1613. Lib. III. Prop. III.

⁴Cartesius: *Dioptrice*. 1637. VI, 11 and 13.

a certain part of our brain in such manner as is instituted by nature to make the mind perceive that distance. . . . Secondly we perceive distance by the relation which the two eyes bear to each other. For consider a blind man, who holds in his hands two converging staffs. Their length he does not know, but knows only the distance between his hands, and the magnitude of the angles which the converging staffs make with an imaginary line joining his two hands. He perceives by a sort of natural geometry (*ex geometria quadam omnibus innata*) the distance of the point of intersection of the staffs. So we, when our eyes are converged upon a point, know the distance of that point by the length of the interocular line and the magnitude of the angles formed at the points of intersection of the interocular line and the visual axes."¹

Descartes' theory of the part played by accommodation lapsed with the lapse of the theory of the mechanism of accommodation which it assumed,—although it is not different in principle from the view dominant in the psychology of a few decades ago. His explanation of the function of convergence was rejected by Berkeley who pointed out² that as a matter of fact we possess no knowledge of the oculo-geometrical relations whose assumption furnished the basis of the Cartesian account.

Malebranche³ enumerates six "signs of distance." The first and second of these are, however, nothing more than Descartes' accommodation and convergence criteria, and the other four do not concern us here. Molyneux⁴ defends the Cartesian theory against the attacks of opponents and brings forward another criterion of distance which has, however, never found favor. He is of opinion that "when we estimate the distance of nigh objects, either we take the help of both eyes, or else we consider the pupil of one eye as having breadth, and receiving a parcel of rays from each radiating point. And, according to the various inclinations of the rays from one point, on the various parts of the pupil, we make our estimate of the distance of the object."⁵

Berkeley⁶ set himself the task of showing "the manner in which we perceive by sight the distance . . . of objects. . . . It is, I think, agreed by all that distance of itself, and immediately, cannot be seen. For distance being a line directed endwise to the eye, it projects only one point in

¹ Cartesius: *Ibid.* VI, 11, 13. Paraphrase.

² G. Berkeley: *An Essay towards a New Theory of Vision*, 1709, § 4, 8-17.

³ N. Malebranche: *Recherche de la Vérité*, 1675, I. 9.

⁴ Wm. Molyneux: *A Treatise on Dioptrics*. Dublin, 1690.

⁵ *Ibid.* Part I.

⁶ *Op. cit.*, § § 1 and 2.

the fund of the eye—which point remains invariably the same, whether the distance be longer or shorter.” He refutes the contentions of Descartes and of Molyneux¹ and maintains that accommodation and convergence furnish a threefold “sign” of the distance of near objects. “First, it is certain by experience, that when we look at a near object with both eyes, according as it approaches or recedes from us, we alter the disposition of our eyes by lessening or widening the interval between the pupils. This disposition or turn of the eyes is attended with a sensation, which seems to me to be that which in this case brings the idea of greater or lesser distance into the mind. . . .

. . . Secondly, an object placed at a certain distance from the eye, to which the breadth of the pupil bears a considerable proportion, being made to approach, is seen more confusedly. And the nearer it is brought the more confused appearance it makes. And, this being found constantly to be so, there arises in the mind an habitual connection between the several degrees of confusion and distance; the greater confusion still implying the lesser distance and the lesser confusion the greater distance of the object. . . . Thirdly, an object being placed at the distance above specified, and brought nearer to the eye, we may nevertheless prevent, at least for some time, the appearance’s growing more confused, by straining the eye. In which case that sensation supplies the place of confused vision, in aiding the mind to judge of the distance of the object; it being esteemed so much the nearer by how much the effort or straining of the eye in order to distinct vision is greater.”²

Berkeley’s Essay was followed by a voluminous criticism—favorable and adverse; but little positive advance was made until the beginning of the last century. Berkeley’s theory was opposed by R. Smith,³ Condillac,⁴ Wm. Porterfield⁵ and others, but was supported by Condillac,⁶ Voltaire,⁷ D. Hartley,⁸ T. Reid,⁹ Adam Smith,¹⁰ Dugald Stewart,¹¹ Thomas Brown,¹² Sir Wm. Hamilton,¹³ A. Bain,¹⁴ John Stuart Mill,¹⁵ and J. Mackintosh.¹⁶

¹ *Ibid.*, § § 4-15 and *Appendix to Second Edition*.

² *Ibid.*, § § 16-27.

³ *A Complete System of Opticks*, Cambridge, 1738.

⁴ *Essais sur l’origine des connaissances humaines*, 1746, I.

⁵ *A Treatise on the Eye, the Manner and Phenomena of Vision*, 1759.

⁶ *Traité des Sensations*, 1754. Condillac was at first opposed to the Berkeleyian theory of vision but later accepted it.

⁷ *Elémens de la philosophie de Newton*, 1738.

⁸ *Observations on Man*, 1749.

⁹ *Inquiry into the Human Mind*, 1763.

¹⁰ *Essays. On the External Senses*, 1795.

¹¹ *Elements of the Philosophy of the Human Mind*, 1792.

¹² *Lectures*, 1811.

¹³ *Lectures on Metaphysics*, 1837; *Reid’s Works*, 1847.

¹⁴ *Senses and Intellect*, 1855.

¹⁵ *Discussion*, 1859.

¹⁶ *Dissertations*, 1862.

The Berkeleyian principle, in so far at least as it concerned convergence, was taken up and extended by Steinbuch in Germany.¹ Steinbuch asserted, of all three dimensions of space, what Berkeley had posited for depth alone,—that their perception is a result of the sensations aroused by the contraction of the ocular muscles. When an object makes its appearance in the field of vision, we run our eyes over its various dimensions; and the amount of spatial extension which we ascribe to it is determined by our consciousness of the amount of muscular contraction employed in successively regarding its several parts.

A novel explanation of the *modus operandi* of depth vision was brought forward by Lehot.² In the opinion of Lehot the retinal image of a solid object itself possesses tridimensionality. This corporeal image, standing upon the retina, projects into the substance of the vitreous humor, and is there sensed as tridimensional by means of the sensitive fibres with which that humor is supplied.

Hueck³ was the first investigator to attack the problem experimentally. He performed a series of experiments which he thought established the fact that ocular movements of convergence influence our judgment of the position and distance of the objects fixated. Besides establishing this relation for the normal eye, Hueck maintained that certain abnormal phenomena in space localization were explicable from the pathological condition of the ocular muscles. A few years later, Meyer⁴ described experiments which seemed to prove that consciousness of change of convergence of the visual axes gives rise to a change in our estimation of the magnitude and distance of visual objects. Meyer's experiments consisted in a successive fixation of points before and behind a surface upon which was printed a series of congruent figures—wall-paper patterns. He found that with near fixation, or increased convergence, the figures appeared to be small and near, while with decreased convergence they seemed to be larger and more remote. In a later and more detailed investigation, Meyer⁵

¹ *Beiträge zur Physiologie der Sinne*. Nürnberg, 1811.

² C. J. Lehot: *Nouvelle théorie de la vision*. Paris, 1823. This is the earliest record we have been able to find of an attempt to explain depth-vision from a purely Nativistic standpoint. Lehot's treatise antedates the work of Johannes Müller—the reputed founder of Nativism—by several years. Moreover Müller's *Vergleichende Physiologie* is, after all, only the modest beginning of a very modest Nativistic theory. It contains no more than the germ of the radical Nativism of his successors.

³ Alex. Hueck: *Die Achsendrehung des Auges*, Dorpat, 1838.

⁴ Hermann Meyer: *Ueber einige Täuschungen in der Entfernung*. *Archiv für physiologische Heilkunde*, 1842.

⁵ Hermann Meyer: *Ueber die Schätzung der Grösse und Entfernung*. *Poggendorff's Annalen der Physik und Chemie*. III, Reihe. XXV, 1852. 198-207.

employed a modified form of the Wheatstone stereoscope. He found that, if two figures were inserted in the slides and combined into a single image, this stereoscopic image appeared to approach and recede from the observer, in the direction of depth, according as the figures were moved back and forth in the slides. He showed by means of a drawing, representing the paths of the reflected rays, that the change of apparent distance corresponded throughout the series with the change of convergence of the visual axes. That is, if with any given position of the figures in the slides, the eyes were required to assume a position of greater convergence in order to combine the images, the single image was projected to a near distance, while, on the other hand, if a farther point must be fixated in order to bring about the blending of the images, the visual object was localized at a greater distance from the eye.

In 1852 Lotze brought forward his well-known theory of "Local Signs."¹ When an object makes its appearance in the field of vision the eyes reflexly turn in such a manner as to bring the images of the object to the part of clearest vision. The ocular movement necessary for the transference of the image from any lateral point to the fovea differs in magnitude and direction for every lateral retinal point. If now these movements are attended by muscular sensations, and if the latter constitute a series of sufficiently fine gradation, every retinal point will furnish in the peculiar movement which corresponds to it and to it alone, a clue for the localization of its objective stimulus. Nor are actual movements necessary, in the opinion of Lotze; the mere tendency to movement may suffice to bring to consciousness a reproduced sensation corresponding to the appropriate movement, and may thereby furnish the local sign.

The Lotzian doctrine was worked out in detail by Meissner.² Meissner assumes that every visual sensation has a breadth, height and depth-value, which are referred to a system of ordinates passing through the point of fixation in external space. The point at which the visual axes intersect the center of this system of ordinates is seen to be the center of the binocular field of vision. This point is the point of reference of the space system; the spatial relations of all other parts of the visual field are evaluated in terms of their distance and direction from this zero-point. The localization of any lateral stimulus in relation to this central point is brought about by the sensation of movement which accompanies the transference of the image to the fovea. Breadth and height-values are furnished in monocular vision; depth-values come to consciousness only in binocular vision.

¹ H. Lotze: *Medicinische Psychologie*, 1852.

² Georg Meissner: *Beiträge zur Physiologie des Sehorgans*, 1854.

In the course of its development since the days of Descartes and Berkeley, this Empiristic Theory, as it is called, had not only been filled out in detail; it had been enormously broadened in scope as well. Berkeley had made the perception of depth a function of muscular sensations. The later and more extreme Empirists assert that all space perception—breadth and height as well as depth—is dependent upon sensations arising from eye-movements. Berkeley had further maintained that the relation between a given visual sign and the distance which it signifies is purely arbitrary, and that this relation is discoverable only by experience. That this principle can be posited of breadth and height in the same sense in which Berkeley held it to be true of depth, can scarcely be maintained. It is unfortunate that many Empirists have evaded this issue.

The Empiristic theory did not long remain in undisputed possession of the field. Beginning, as is commonly supposed, with Johannes Müller, there arose the rival theory of Nativism, whose development we must now trace. The terms Empirism and Nativism are unhappily chosen. Neither the Nativist nor the Empirist would subscribe to a rigid formulation of any such doctrine as his title implies. For the latter is as far from holding that our earliest experience is absolutely non-spatial in character as is the former from believing that our adult spatial vision is a connate endowment.

Johannes Müller¹ combats the view advanced by Steinbuch, objecting that the latter's argument is unsound, in that he derives space perception from a consciousness of movements which itself presupposes the idea of space. Müller, on the contrary, believes that the capacity to see space is an innate endowment of the retina. Even when the eyes are closed and unmoved, the retina "sees itself extended." Yet Müller believes that eye movements are an important secondary factor in the development of our adult perception of space.

A new impulse was given to the Nativistic theory by Dove's refutation of Brücke's view. A few years previously, Wheatstone's² earliest stereoscopic experiments had convinced their author of the erroneousness of the theory of identical retinal points. It had long been held that the explanation of the circumstance that though we see with two eyes yet we are conscious of but a single image, was to be found in the fact of the paired arrangement of retinal points. Every point on the one retina has its mate on the other; and it is characteristic of

¹Johannes Müller: *Vergleichende Physiologie des Gesichtsinnes*, 1826, pp. 52 ff.

²Charles Wheatstone: *Contributions to the Physiology of Vision*, Philosophical Transactions, 1838, pp. 371-394.

these identical points, that the simultaneous stimulation of any pair gives rise to an unitary perception. Wheatstone, however, found an essential condition of stereoscopic vision in the disparity of retinal images. Since it is impossible that two dissimilar projections of an object can be imaged at the same time upon similar or identical parts of the two retinas, and since, moreover, two dissimilar projections may give rise to a single image of the object, Wheatstone felt himself obliged to reject the theory of identity. But he was also obliged to rest content with this half-way measure; for he was unable to advance a satisfactory hypothesis to replace the old. Brücke,¹ doubtless at the instigation of the horopter discussions in the literature of that period, found a solution of the problem which at once saved the theory of identity and explained the phenomena of stereoscopic vision. Since only those points which lie upon the momentary horopter can at any one instant be seen singly and clearly, Brücke finds it necessary to assume that the normal procedure in seeing a solid object consists essentially in a rapid series of eye-movements, during the course of which the various parts of the object are successively fixated. By this means, the parts are successively imaged upon identical retinal points; and meanwhile the different distances of the parts, *i. e.*, the tridimensionality of the object, is perceived from the sensations aroused by the movements of convergence. When we regard stereoscopic pictures, a similar series of eye-movements exposes identical points on the two retinas to the various parts of the object fixated, and the consequent perception of relief is, here too, to be explained from sensations of convergence.

Dove² showed that it is possible to get the stereoscopic effect with an instantaneous exposure of the stereograms.³ Since his exposure was computed to have a duration of less than the ten millionth part of a second, we are left to conclude, either that the eye-movements occur with inconceivable rapidity or that they are not an essential condition of stereoscopic vision. It is a surprising fact that notwithstanding Dove's demonstration, Brücke's theory was still accepted without question by several subsequent writers, *e. g.*, Prevost, Brewster and Abbott.

Panum, however,⁴ saw in Dove's demonstration a telling

¹ Ernst Brücke: *Ueber die stereoskopischen Erscheinungen u. s. w. Müller's Archiv f. Anatomie*, 1841, p. 459.

² H. A. Dove: *Berichte d. Berliner Akademie*, 1841, p. 252.

³ This demonstration has since been repeated in various forms by Volkmann, Panum, Donders, Aubert and others. The most familiar modern form is that described by Hering in connection with his falling ball apparatus. *Arch. f. Anat., Physiol. u. wiss. Med., Leipzig*, 1865, p. 512.

⁴ P. L. Panum: *Physiologische Untersuchungen über das Sehen mit zwei Augen*, 1858. The writer has been unable to gain access to a

argument against the Empiristic doctrine of psychological space. The theory which he advocates is a Nativism of the most pronounced type. It is true that Johannes Müller had made spatial vision an innate capacity of the retina; but Müller had brought forward only a skeleton theory, and had but vaguely indicated the course of the ontogenetic development of visual space perception which he advocated: Panum attacked the problem more resolutely, and if he did not find a satisfactory solution, at least made a valuable contribution to the literature, and directed the discussion into a path which led his successors to fruitful results. If, as Dove's experiment seemed to show, sensations of movement are a non-essential in spatial vision, it is only natural that Panum should seek to find, in the retina itself, the conditions which are necessary. Accordingly Panum was led back to Müller's hypothesis and himself maintained that visual space is a function solely of the retina and its central nervous mechanism. For Panum the spatial position of a visual object is given us immediately in sensation. This specific sensation of space is conditioned by the position of the image upon the retina. The sensation of depth, which is a product of the simultaneous co-operation of the two retinas he calls "the sensation of binocular parallax." If, now, a characteristic space sensation corresponding to each individual retinal point is aroused simultaneously with the stimulation of that point, and if, moreover, this space-sensation suffices for the localization of the visual sensation at a point in space coincident in all three dimensions with the position of the stimulus, we have a simple and satisfactory solution of the problem. Eye-movements and eye-movement sensations alike become unnecessary and superfluous. But if we ask just what constitutes the differentiation between the several space-sensations belonging to the various retinal points, *i. e.*, if we seek for criteria which may take the place of Lotze's movements and movement tendencies or Steinbuch's "muscle-ideas" in rendering the whole procedure possible or comprehensible, we find that Nativism, even in the hands of Panum, has failed to penetrate the mystery.

The modern form of the Nativistic Theory, as formulated by Hering¹ is, strangely enough, a mixed descendant of the theories of Johannes Müller, Lotze, Meissner, Nagel and Panum. Its assumption of specific space-sensations as constituting the

copy of this work. His knowledge of Panum's position has been obtained at second-hand from the numerous discussions in the literature.

¹ Ewald Hering: *Beiträge zur Physiologie*, 1861-4; *Die Lehre vom binocularen Sehen*, 1868; *Raumsinn des Auges*, in Hermann's Handbuch der Physiologie, 1879, III, p. 343.

local coloring of the visual image is a characteristic of the theories of Müller and Panum; its nomenclature had already been given to the world by Meissner, and its advocacy of the dependence of the space-value (or local sign) upon distance and direction from the retinal meridians is also found in Lotze and Meissner. However, it must be recognized that Hering has worked out the Nativistic theory with greater detail than any of his predecessors; and if in its present form the theory has suffered most from hostile criticism, this is doubtless due to the fact that the modern form can be more clearly envisaged and its inherent nativistic limitations more clearly seen.

The theories of Lotze and of Hering have in common the assumption that to every retinal point belongs its own space-value; and in both theories this space-value is a function of the distance and direction of the point in question from the *fovea centralis*. But whereas Lotze assumes that the space-values (local signs) come to consciousness *indirectly*, *i. e.*, through the mediation of the sensation aroused by the movement required to transfer the image from that point to the fovea, Hering assumes that space-values come *directly* to consciousness, as sensations in their own right. To Hering, then, the stimulation of any retinal point gives rise to a two-fold sensation: a light sensation, whose attributes are essentially determined by the character of the objective stimulus, and a space-feeling, whose nature is determined by the position of the retinal point stimulated. Nagel had called attention to the familiar fact that the position of a point in external space is determined when we have determined its direction and distance from the fovea. But whereas Nagel derived our determination of these two spatial relations from sense-data,¹ Hering posits specific retinal sensations of direction and depth. The former he again subdivides into feelings of breadth and of height; so that the space-feeling which arises on the stimulation of any retinal point has a three-fold content: a feeling of breadth, of height and of depth. The fovea, as for Lotze and Meissner, is the central point or point of reference of the whole space-system, and itself possesses a space-value = 0. The space-value which accrues to an individual retinal point, in virtue of the space-feelings aroused on the stimulation of the point, are not absolute but relative only. Hering's space-feelings therefore provide merely for relative space-localization,—localization in relation to a given nuclear point of visual space. This nuclear point (*Kernpunkt*) coincides approximately with the momentary point of fixation.

This theory makes no use of eye-movements in its account of the origin of spatial vision. Indeed, Hering explicitly

¹A. Nagel: *Das Sehen mit zwei Augen*, 1861, pp. 178 ff.

asserts that, in binocular vision at least, eye-movements are the effect, not the cause, of the perception of depth.¹

Moreover, since the seeing of depth is, for Hering, a function of the relative position of the images upon the two retinas, it is evident that his theory is unable to explain the monocular estimation of depth. Since Hering follows Panum in making depth vision a product of the simultaneous co-operation of the two retinas, he is forced either to deny to monocular vision the possibility of estimating depth, or to eke out his theory by supplementary hypotheses in order to account for this possibility. Of the two evils he chooses the latter; but in so doing he is forced to the damaging admission that the difference between monocular and binocular depth estimation is not one of degree only, but is an essential and absolute difference. The latter is immediate, while the former is mediated by accommodation;² they are as distinct from each other as are the processes of sensation and of inference.

Besides the Nativistic and Empiristic theories discussed above, there has also been advanced a Genetic theory of visual space. The latter is an off-shoot from the Empiristic stem; its origin may be traced to a series of experiments performed by Wundt during the years 1858 to 1862.³ The object of Wundt's investigation was to determine the influence of accommodation and convergence upon the perception of depth. In his experiments, a fine black silk thread was suspended vertically at a point between an observation-tube and a distant white background. Experiments were made in both binocular and monocular vision, the same method being employed throughout both series. The observer looked through the tube, and, after fixating the thread in its first position, turned his head while the thread was being moved to a new position. He again looked through the tube, fixated the distant background, then refixated the thread, and judged whether the movement of the latter had been in the direction of approach or recession. In this way were determined the limens of approach and recession for a series of distances ranging from 40 cm. to 250 cm. The monocular experiments yielded two general results: (1) The observer was unable to form any definite estimate of absolute distance, but could perceive changes of distance within certain limits; (2) the limens of recession were invariably greater than those of approach. The binocular experiments showed less indefiniteness in the estimation of absolute distance.

¹ *Beiträge*, p. 320.

² *Raumsinn des Auges*, p. 546.

³ *Zeitschrift für rationelle Medizin von Henle und Pfeufer*. These papers were collected and published under the title, *Beiträge zur Theorie der Sinneswahrnehmung*, 1862.

ces, though the estimate invariably fell short of the actual distance (ranging from $\frac{1}{3}$ to $\frac{1}{2}$ of the actual distance). Here, too, however, differences of distance were correctly estimated within certain limits, and these limits were considerably narrower than those in monocular vision. Indeed, the results indicated a 2.5 to 4.5 times greater sensitivity to depth in binocular vision. The difference between the limens of approach and recession vanished in the binocular series. Wundt concluded that, in the latter series, the comparison of two distances was a comparison of two convergence positions by means of muscular sensations, and that the perception of distance was essentially conditioned by this sensation factor. He put a different interpretation, however, upon the monocular results. Since accommodation from a farther to a nearer point is accomplished by a muscle contraction, and may, therefore, be assumed to be attended by a muscular sensation, while accommodation from a nearer to a farther point is accomplished by a release of muscle contraction, and cannot, therefore, give rise to any muscular sensation, Wundt concluded that accommodation is capable of contributing to the perception of change of distance only in the direction of approach. He was of the opinion that, in his monocular experiments, the perception of the recession of the thread was rendered possible solely by the change in the size of the visual angle subtended by the receding thread.

The net results, then, of Wundt's investigation are his conclusions that in binocular vision the perception of approach and recession is the resultant of sensations of convergence, while in monocular vision the perception of approach alone is the resultant of sensations of accommodation.

Wundt is essentially an orthodox Empirist in his interpretation of these results. In the opinion of the earlier advocates of Empirism, our knowledge of spatial relation is the product of a distinctly conscious process. The 'visual sign' is present in consciousness as such; the judgment of the distance signified in a conscious inference. The substitution of an unconscious inference made its appearance in the later forms of the theory. This was especially characteristic of the view of Helmholtz and was characteristic also of Wundt's earlier theory. "With the accommodation is associated a feeling in the eye from which an inference is drawn as to the approach of the observed object."¹

The Scottish school assumed a process of association to explain the origin of visual ideas of space. Thus, in the system of Bain, the association of retinal sensations with sensations of

¹ Wundt: *Beiträge*, p. 109.

different intensity arising from ocular movements, gives rise to ideas of linear and two-dimensional space. The perception of depth is traced to an association of retinal sensations with sensations of convergence and accommodation.

In his later writings on psychological space Wundt modifies his earlier view. His theory of Complex Local Signs assumes a characteristic psychical process to account for the perception of space.

Meanwhile the experimental investigation was continued with renewed vigor. Helmholtz¹ describes an illusion which shows the influence of accommodation upon the estimation of distance. "At the end of a tube, blackened on the inside, I set up a screen pierced by two vertical slits which were covered, the one with a red and the other with a blue glass. A noticeably greater effort of accommodation was required to see the red line distinctly than was the case with the blue. Finally after numerous trials I got the impression that the red line stood out nearer than the blue."

In 1894 Hillebrand contributed a paper to the discussion.² Hillebrand objected that binocular experiments are inadequate to a solution of the problem since the use of two eyes introduces secondary criteria, notably crossed and uncrossed double images, and thus prevents an isolation of the influence of the factors of accommodation and convergence. His experiments dealt with monocular vision alone. They were concerned chiefly with an investigation of the relation between accommodation and depth localization; but, since accommodation and convergence are intimately associated physiologically, the influence of convergence was not excluded. He also objected to the employment of a thread as fixation-object, since the change in its apparent diameter with change of distance and inequalities in the thread itself may furnish a clue to distance estimation, and thus defeat the end of the investigation. In Hillebrand's apparatus the clean-cut edge of a black cardboard screen was brought into the median plane of the field of vision,—the screen thus hiding half of the distant white background. The mathematical line which marked the boundary between the black and the white halves of the field of vision served as fixation-object. A mechanical device enabled him to expose this fixation-object at any distance (up to one meter) from the observer, to move it gradually towards or from the observer, and to remove it abruptly from the visual field and to replace it immediately by another similar screen-edge at a

¹ H. Helmholtz: *Phys. Optik*, 1867, 634; 1897, 779.

² F. Hillebrand: *Das Verhältniss von Accommodation und Convergence zur Tiefenlokalization*. Zeitschrift für Psychologie und Physiologie der Sinnesorgane, VII, 1894, p. 98.

different distance.¹ Hillebrand's experiments assumed two general forms. In the first series the screen was gradually moved towards or from the observer, the sliding movement being begun before the screen was exposed. The movement itself was constant and its rapidity was so gauged that the observer could conveniently follow with his accommodation; *i. e.*, the movement was so slow that change of accommodation could readily keep pace with it; dispersion circles were, therefore, never present in any considerable degree. In the second series two screens were employed; after the first had been fixated for a time, it was *abruptly* removed laterally from the field of vision, and the other was immediately brought in at a different distance, the observer being required to state whether in the second case the distance was greater or less than in the first.

When the change of distance was *gradual*, the observers were unable to state the direction of the movement of the fixation-object, although it seems clear that changes of accommodation must have occurred during the movement. From the negative character of the results of this series, Hillebrand concluded that muscular sensations of accommodation either are non-existent, or at least are inoperative in the perception of depth. For this conclusion he finds additional confirmation in the results of his experiments with an Aubert diaphragm. This instrument he fastened to the screen of his apparatus, and in a series of experiments, he found that, if the aperture in the diaphragm was continuously increased or decreased while its distance from the observer remained constant, a distinct impression of recession or approach arose, although the accommodation had meanwhile remained unchanged. When the diaphragm gradually approached, its aperture meanwhile being *rapidly* decreased, there arose an impression of recession, notwithstanding the greater tension of accommodation.

In the series where the distance of the fixation-object was *abruptly* changed, he found distance-limens, before which and beyond which approach and recession were perceived almost without error. These limens are invariably much larger than those of Wundt, ranging from one to two diopter-differences for approach, and from one to four diopter-differences for recession. The excess of recession limens over those of approach which Wundt's monocular experiments showed, was found with only three of Hillebrand's five observers.

How are these correct estimates of relative distance to be

¹ For cut and detailed description of Hillebrand's apparatus, see his paper, *loc. cit.*, p. 108. The apparatus employed in our own experiments was modelled after Hillebrand's. It is figured and described below, p. 170 ff.

explained? Hillebrand admits that the assumption of accommodation-sensations would furnish a satisfactory explanation, were it not for the fact that his observers were unable to estimate gradual changes of distance. In view of this latter fact he considers the existence of accommodation-sensations as exceedingly problematic and their influence as inoperative. He therefore rejects Wundt's theory and casts about for a more satisfactory explanation. From the introspections of his observers he forms a conjecture as to the manner in which any required adjustment of accommodation is made. This conjecture he confirms by experiment and advances as an explanation of his results.

This explanation is as follows: When a point, which is not equidistant with the point of fixation, appears in the field of vision, it images itself upon the retina in dispersion circles. The observer forthwith sets about to effect that adjustment of accommodation which will give rise to clear vision. But he does not know as yet whether greater or less tension of accommodation is the appropriate movement. Either of the two possible movements is chosen at random and deliberately innervated, the effect upon the definition of the retinal image being noted meanwhile. If an improvement in definition follows, the movement is continued until perfect definition results. If, however, the dispersion images are increased by the initial movement, it is abandoned and the opposite movement is chosen and continued until clear vision is attained. Hillebrand performed a series of experiments whose results showed that adjustment of accommodation for a second point requires less time when the observer knows whether it is nearer or farther than the fixation point, than is required when its direction from the fixation point is unknown. This confirms his suspicions as to the manner in which adjustment of accommodation is made, the excess of time required in the case of unknown direction representing the time lost in experimental tests which result in the appropriate movement being finally hit upon. Accordingly, Hillebrand concludes that the conscious impulse of will (*bewusster Willenimpuls*) which innervates accurate adjustment of accommodation from a first to a second fixation point, is the determining factor in the relative depth localization of the second point.

The net results, then, of Hillebrand's investigations are as follows: Muscular sensations of convergence and accommodation do not contribute to depth localization. The adjustment of accommodation from one point to another is accomplished by an impulse of will. And it is the consciousness of this impulse of will which determines the relative depth localization of the second point.

In 1895 Dixon published a paper describing a series of ex-

periments which he had made with a modified form of Hillebrand's apparatus.¹ In the latter's experiments, the normal and the comparative fixation-objects were imaged upon opposite sides of the retina. In order to remove this objection, and also to shorten the time between their exposures, Dixon devised a modification of the original apparatus which exposed both screens at the same side of the field of vision. Dixon finds no essential difference in principle between Hillebrand's gradual and abrupt experiments. Since the latter form seems better adapted to give definite results, he employed the abrupt change of distance almost exclusively in his investigation. His experimental results agree in the main with those of Hillebrand, but his conclusion is widely different. Each of his observers was able to estimate changes of distance correctly but the capacity varied greatly in different individuals. Wundt's observation, that changes from far to near were more accurately perceived than changes from near to far, was not confirmed in every case. Introspection showed that in all observers "the actual criterion of depth was a difference in the rapidity or ease with which the accommodation adjusted itself (or was adjusted by the observer) and not in any conscious direction of the accommodation by the observer." He is not convinced of the soundness of Hillebrand's argument against the muscular sensation theory of depth perception, though he himself makes no use of muscular sensations in accounting for the depth localization of his own observers.

In 1896 Arrer published new experimental data together with an historical and critical discussion of the whole problem.² Arrer's apparatus and method differed little from those of Wundt. Indeed, such modifications as were introduced were made at the suggestion of Wundt himself. Threads were employed as fixation-objects; experiments were made in both binocular and monocular vision. Arrer also repeated Hillebrand's experiments with a duplicate of the latter's apparatus and obtained similar results. Two objectionable features of Hillebrand's apparatus were pointed out by Arrer: the juxtaposition of the white and the black portions of the field of vision gives rise to such a degree of contrast and irradiation as renders accurate accommodation uncertain if not impossible. Moreover, the fixation of a mathematical line brings with it no definite idea of its absolute depth, and Arrer is convinced from his investigation that without a definite presentation of abso-

¹E. T. Dixon, On the Relation of Accommodation and Convergence to our Sense of Depth, *Mind*, N. S., IV, 1895, p. 195.

²M. Arrer: *Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung*, *Phil. Stud.*, XIII, 1896-7, p. 116 ff.; p. 222 ff.

lute distance, perception of relative distance is impossible. Accordingly, Hillebrand's apparatus is rejected as being inadequate to a positive solution of the problem. Arrer defends his binocular experiments against Hillebrand's attack, maintaining that, as a matter of fact, double images were invariably lacking from his experiments, nor did their intentional production furnish anything but a disturbing factor. He also defends the use of threads as fixation-objects. A mathematical determination of the amount of variation of visual angle with approach and recession of the thread in his experiments convinces him that this variation falls below the minimal values which have been determined for the just observable difference of visual angle.

Arrer's monocular experiments gave a somewhat greater liminal value than his binocular, though monocular perception of difference of depth was fairly definite. Wundt's difference of limen for approach and recession was not a characteristic of all observers.

Arrer concluded that Wundt's explanation of the perception of monocular recession is not valid. He ascribes to convergence sensations the leading rôle in depth perception in his monocular as well as in his binocular experiments. The closed eye converges, approximately at least, upon the fixation point, probably being guided in its movement by the accommodation of the seeing eye. This indirect and problematic influence seems to be the sole factor which Arrer conceives to be contributed to depth perception by accommodation.

The net result of Arrer's investigation is his conclusion that the sense-factors of depth-localization, absolute and relative alike, are the sensations of convergence and accommodation. The estimation of depth occurred neither through an immediate perception of the degree of convergence strain, nor through convergence-sensations being experientially associated with the object to be localized. Sensations of convergence and accommodation are, however, those elements in our ideas of space by which reference to depth is conditioned for consciousness.

In 1897, Hillebrand contributed a second paper to the discussion.¹ In view of the fact that the experimental results which he had previously published were subsequently confirmed by Arrer and by Dixon, he finds it unnecessary to bring forward new experimental data. He now reviews the history of the problem and discusses its essential features and immediate bearings upon space theories in general. He restates his objections to the method and apparatus of Wundt and Arrer, criticises

¹F. Hillebrand: *In Sachen der optischen Tiefenlocalization*, *Zeits. f. Psych.*, XV, 1897, p. 70.

the conclusions of Wundt, Arrer and Dixon, discusses and defends Hering's theory of space, meets the objections raised by his critics and intrenches himself in the position assumed in his previous paper.

In his most recent contribution to the psychology of visual space, Wundt devotes a section of his paper to a discussion of this problem.¹ He meets Hillebrand's objections to his earlier experiments—the influence of double images and change of visual angle—and discusses the possibility of explaining his experiments from convergence alone. He, in turn, objects that Hillebrand's fixation-object was not really a mathematical line, but an indefinite band, shading off from white into black. Accurate accommodation upon this band is impossible, hence the negative character of the results obtained by Hillebrand. It is true that, in the abrupt series, a relative localization of this fixation-object was possible, but there is evidence in the results of Dixon and Arrer that this possibility was due to the presence of secondary criteria of depth. Since these criteria were doubtless introduced because of the absence of normal conditions of accommodation, the inadequacy of Hillebrand's experimental conditions for a positive solution of the problem is evident.

Wundt further maintains that Hillebrand's theory of voluntary innervation does not furnish a satisfactory explanation of depth-localization. The assumption that eye-movements are a pure function of the will is in direct opposition to familiar facts of experience—to the difficulty of isolating a given position of accommodation from its concomitant position of convergence, and to the well known tendency of the eye to turn towards the electric spark in dark-room experiments.

Wundt's and Arrer's results indicate that a much more definite idea of distance is associated with convergence than with accommodation. But just what is the relative contribution of each to the resultant perception of depth must remain for the present an open question.

According to Wundt, the sensations arising from eye-movements are not merely muscular, but are also tendinous and pseudoarticular,—the latter arising from the rotation of the eye-ball in its socket. In small excursions these sensations are not present to consciousness, as such, simply because they fuse immediately with other sensations to form perceptions of space. An analogy is found in the case of arm-movements. When the excursion of arm-movement is small, the movement is still perceived, though the joint sensations are not present as such.

¹Wundt: *Zur Theorie der visuellen Raumwahrnehmung*, *Phil. Stud.*, XIV, 1898, p. 1, esply. pp. 11-16.

This is but an example of a law that holds for all sense-perception; namely, that sensations of moderate intensity fuse completely to form perceptions. The formation of complex spatial ideas from sensations of ocular movements is, then, but a case of psychical assimilation.

Wundt's present theory¹ is a product of many years' growth. Wundt is of opinion that the idea of space cannot be assumed to arise from light sensations in themselves. The spatial order is the resultant of a combination of sense-data which taken separately possess no spatial attributes. His theory of Complex Local Signs takes its origin from the fact that the eye is at once an organ of vision and of movement. Each of these modes of functioning furnishes a system of local signs; and by local sign Wundt means any datum for consciousness which is effective in the localization of an impression. It is a well-known fact that the same stimulus may arouse qualitatively different sensations at different parts of the retina. Accordingly, Wundt assumes that every sensation possesses its own peculiar local coloring, corresponding to the part of the retina stimulated. These qualitative differences constitute a first system of local signs. Since the movements of the eye are attended by sensations of muscular and orbital origin, they are able to furnish a second system of local signs. Moreover, these two systems are brought into intimate relation by the reflex transference of eccentric impressions to the center of the retina; consequently they may be regarded as a single system of complex local signs. The psychical process which gives rise to the perception of space is a fusion. And it is characteristic of the fusion that the component sense-data are so closely merged in a single product that they are not discernible as sense-data. In the opinion of Wundt, then, eye-movements are an essential factor in the perception of space. Changes of convergence are accompanied by sensations which constitute an important element in the binocular perception of depth; accommodation furnishes sensations which, though assigned a minor rôle, come into play in the monocular perception of depth.

An investigation into the visual perception of space has recently been made by Bourdon.² In addition to accommodation and convergence Bourdon mentions, as monocular depth factors, the nodal and pupillary parallaxes and the monocular parallax. The latter refers to changes in the position of the

¹Wundt: *Logik*, 1880, pp. 437 ff., *Outlines of Psychology*, 2nd. English Edition, 1902, pp. 113 ff., *Grundzüge der physiologischen Psychologie*, 5th Edition, 1903, II, pp. 501 ff.

²B. Bourdon: *La perception visuelle de la profondeur*, *Revue philosophique*, 1898, XLVI, pp. 124 ff. *La perception visuelle de l'espace*. Paris, 1902.

eye as a result of movements of the head; by the pupillary and nodal parallaxes are designated changes in the position of the pupil and nodal points relatively to the position of the visual object. Bourdon attaches but slight importance to accommodation as a factor in the monocular perception of depth. Binocular convergence occurs even in monocular vision and furnishes a clue to distance. The pupillary and nodal parallaxes give only a very imperfect knowledge of depth. Of all the possible factors, the monocular parallax is the most important in the monocular perception of distance. Bourdon publishes the results of experiments which he performed with a view to determining the influence of convergence and accommodation in the monocular estimation of distance. In the comparison of two distances—2 m. and 6.5 m.—successively marked off by electric sparks, he found that changes in the direction of approach are much more accurately perceived than changes in the opposite direction. When the two distances were given simultaneously, the results were negative. The absolute estimation of distance is extremely uncertain in monocular vision.

In his first series of binocular experiments two luminous objects appeared successively in the observer's median plane. The standard distance was 1 m. and the comparative distances 1.08 m., 1.12 m. . . . up to 1.32 m. His results show that for one subject the differential limen was less than .08 while for the other it lay between .16 and .20. In his second series of binocular experiments, he employed distances ranging from 10 m. to 25m.; lanterns at the ends of a dark L-shaped passage-way served as fixation-objects. The observer stood at the intersection of the two arms of the passage-way; after fixating the first lantern he turned on his heel through 90° and fixated the second. The results show a preponderance of right judgments only when the ratio of the two distances was 1:2 or more. An obvious source of error is to be found in the crudeness of the method employed in the second series of binocular experiments. Since the observer was directly opposite a corner of the wall and but a few feet removed from it, there is no guarantee that the first position of convergence and accommodation was not lost and forgotten before the second was assumed. Moreover the most zealous advocates of the influence of convergence and accommodation have never maintained that this influence is operative save in the perception of near distances.

An historical survey of the literature of depth-perception reveals the existence and progressive development of two distinct types of theory.¹ The one dates from Descartes and

¹ Wundt proposes a different classification. *Logik*, pp. 452 ff.

Berkeley and culminates in Helmholtz; the other may be traced from Johannes Müller to Hering. The genetic theory of Wundt occupies, to some degree, a mediating position between the two extremes. Of these three later views, only those of Hering and of Wundt are discutable at the present day. Original as these men are, they represent theories which contain no essential element—save only Wundt's conception of the nature of the space-fusion—which is not considerably older than its modern advocates.

The problem of the visual perception of depth has been before the world for nearly four centuries; yet, though it has engaged the attention of the best scientific minds of that period, in only four instances—Wundt, Hillebrand, Arrer and Dixon—has it been submitted to even an approximately adequate experimental test. If our results are, as we believe they are, decisive, they furnish yet another instance of the celerity and certainty with which the experimental method can deal with disputes of long standing.

The present status of the problem may be summarized as follows:

The only point upon which agreement has been reached is the bare general fact that the motor adjustment of the visual mechanism contributes to depth-localization. As to what is the character of this contribution, and the manner in which it is made, there is, as we have seen, a wide diversity of opinion.

In the opinion of Hillebrand, we must ascribe to accommodation the rôle of furnishing dispersion images, and of occasioning thereby a voluntary impulse to innervate a readjustment of accommodation, appropriate to secure perfect definition. This conscious adjustment of accommodation (or, more properly, the innervation which ushers it in) is the determining factor in the monocular depth-localization of the fixation-point, under the conditions of his experiments.

According to Dixon, the difference in the degree of rapidity or ease with which the readjustment of accommodation came about was the actual criterion of depth in his experiments. According to Wundt and Arrer, changes of convergence and concomitant changes of accommodation furnish muscular sensations which, in turn, constitute the sense-factors of our perceptions of tridimensional space.

II. EXPERIMENTAL.

i. *Apparatus.* The possibility of solving this problem experimentally reduces, in the last analysis, to the possibility of devising a means of isolating the influence of the convergence and accommodation factor, and of determining how, and in

what degree, this factor, unaided by any other depth criterion, is capable of contributing to depth-localization. Hence a fundamental requisite of the apparatus to be employed is that it shall render inoperative all depth criteria save only that of accommodation and convergence. It is, of course, essential that depth-localization by means of accommodation and convergence shall be possible under the conditions of the experiment.

The form of apparatus devised by Hillebrand seems to meet the requirements of the present investigation. The exclusion of binocular vision is the simplest means of eliminating the criterion furnished by disparity of retinal images. The co-operation of the factor arising from change of size of visual angle is most satisfactorily ruled out by choosing for fixation-object an object whose size does not change with change of distance. It is true that Wundt and Arrer have raised serious objections against this apparatus. They found that the influence of irradiation and contrast renders Hillebrand's fixation-object so indefinite and obscure that accurate accommodation upon it was impossible. This may, conceivably, be the case with strong degrees of illumination-intensity of the background. But with moderate intensity of illumination, it is certainly not the case.¹ No one of our five observers, no one of the numerous visitors who interested themselves in the apparatus, ever failed to see the screen-edge as a clear-cut, well-defined line, or experienced the slightest difficulty in accommodating accurately upon it, so long as the screen stood beyond the near-point of accommodation. That this objection is invalid, that accommodation, under Hillebrand's experimental conditions, is possible and practicable, is abundantly proved by the accurate distance-estimates of which our observers were capable.

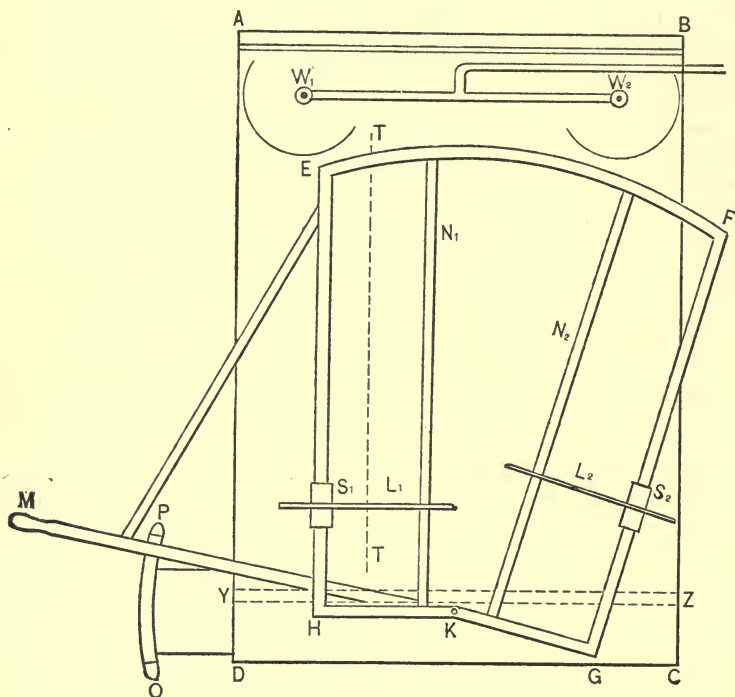
To Hillebrand, then, we are indebted for the apparatus.² We have modified it only in non-essential details.

In Figure 1, ABCD is a table 130 cm. wide and 140 cm. long. At AB is erected a vertical sheet of ground opal glass (80 cm. by 100 cm.), which serves as the background. EFGH is a stout wooden frame, pivoted to the table at K, and suspended at E and F from a 25 foot ceiling. On EH and FG are fitted slides, S_1 and S_2 , each carrying a vertical screen, L_1 and L_2 . A lever, M, is attached to the swinging wooden frame at H, and secured by a brace from E. A wooden arc, OP, is fastened to the side of the table, and provided with padded blocks at O

¹The intensity of the light reflected to the diaphragm of our observation-tube was equal to the direct light of a standard candle at a distance of 56 cm.

²For Hillebrand's figure and description, see his first article, *Zeits.*, VII, 108 ff.

and P. These blocks are so adjusted that when the lever is pressed against O, the edge of the screen L_2 just comes to the longitudinal center of the table, and when the lever is brought up against P, the edge of L_1 is swung to the center of the table.



On the under side of the lever is fitted a padded block, provided with a spring so arranged as to press against the wooden arc, and thus to lock the swinging frame when it has reached the end of its excursion. The suspension of the frame from the ceiling, and the heavy padding of the stop-blocks render the operation of the apparatus noiseless. A shelf-like attachment, fastened to the screen YZ, a few inches above, and parallel to, the table, incloses the part of the apparatus which projects under the screen. The background is illuminated by two Welsbach gas burners, W_1 and W_2 , shaded by vertical semi-cylindrical shades of blackened tin. A vertical screen (80 cm. by 100 cm.) is built across the table at YZ. At a convenient height, a brass observation-tube is let into this screen. This tube, which is lined with black velvet, is 40 mm. in diameter and 50 mm. long. Its end farthest from the observer is

provided with a diaphragm containing an aperture 10 mm. wide and 15 mm. high. The tube projects from the screen towards the observer, and is so adjusted that, when the observer is in position, the center of rotation of his eye is vertically over the pivot, K. The inner side of the screen YZ is covered with black velvet to prevent reflection of light to the front of the observation screens, L_1 and L_2 . It is also provided with an automatic shutter which closes the diaphragm of the observation-tube. On the outer side of the screen YZ an adjustable chin-rest is fitted. The wooden bars N_1 and N_2 are inserted for the purpose of supporting the bases of the screens L_1 and L_2 , thereby ensuring a vertical position of the screen edges.

The most carefully constructed parts of the apparatus are the screens, L_1 and L_2 . These screens are 32 cm. wide, 75 cm. high and 8 mm. thick; they are constructed of veneered wood to prevent warping. Along the inner edge of each screen is fitted an L-shaped strip of brass. After the brass had been fastened to the screen, its free edge was carefully worked down by a surface plate to a perfect straight-edge. Then the surface of the screen where the brass and the wood are joined was ground down to an uniform surface, filled with molten wax, painted, sandpapered and repainted until it presented a uniform dead-black surface. The joint was barely perceptible in daylight; it certainly could not be detected in the dark-room where the experiments were performed.

ii. *Method.* The object of these experiments was to determine the limens of approach and recession of the fixation-object for various distances, ranging from 286 mm. (3.5 diopters) to 666 mm. (1.5 diopters). The experiments assumed two general forms: (1) with *abrupt* change of distance, and (2) with *gradual* change of distance.

(1) *With abrupt change of distance.* The method employed in this series of experiments was a serial method, without knowledge. The observer seated himself comfortably before the observation-tube; after a pause of from seven to ten minutes for adaptation, he received a signal and brought his eye to the observation-tube. The shutter was opened, and he was asked to assume such a position that half of his field of vision was black and half was white. Then he was required to fixate the screen-edge carefully. When he had secured perfect accommodation upon the fixation-object, he gave a signal; the experimenter grasped the lever, and swung the frame until the lever touched the block at the other end of the wooden arc, thus bringing the second screen-edge to the exact center of the observer's field of vision, but at a different distance.¹ The observer fixated it,

¹Dixon believes that the interval of time elapsing between the removal of the first screen and the exposure of the second was too

and judged whether it was nearer or farther than the first. Then the shutter was closed, introspections were recorded, and after a brief pause the experiment was continued. The observer was cautioned throughout to adjust his head so that the fixation-object was imaged upon the central vertical meridian of his retina, to move neither his head nor his eye during the observation and to announce the first symptom of fatigue.

In this manner the limens of approach and recession were determined for five distances,—286 mm. (3.5 D), 333 mm. (3.0 D), 400 mm. (2.5 D), 500 mm. (2.0 D) and 666 mm. (1.5 D). Ten liminal values of approach and ten of recession were determined for each of the five distances. The average limens, with the mean variations will be found in the tabulated results.

(2) *With gradual change of distance.* A slight modification of the apparatus was found to be necessary for these experiments. The method employed was as follows: the shutter was opened and the observer fixated the screen-edge. When he had acquired a perfect accommodation, he signalled, whereupon the experimenter pushed the screen towards or from him,—the movement being constant and of such rapidity that accommodation could follow it without difficulty. The movement was made by hand, but a considerable degree of constancy and uniformity was soon acquired. The rapidity used in these experiments was approximately 10 cm. in 7 seconds.

It was found early in this series that the sound of the moving slide furnished the observer with a clue to the direction of the movement. Even when the shutter was closed, some of the observers were able, from sound alone, to estimate the direction of the movement with surprising accuracy. Since the presence of this factor would render impossible the isolation of the criteria with which we are concerned, the following plan was devised.

The wooden frame EFGH (Figure 1) was removed from the apparatus. One of the screens, S_1 , was taken from its slide and mounted upon a carriage with grooved wheels. A smooth, hardwood track was constructed upon the table (along the dotted line, T) and the carriage set upon it. Our first plan included a pair of parallel tracks upon the table, but it was found impossible to make them sufficiently smooth for our purpose. An almost infinitesimal tremor of the moving car

long and thus tended to render judgments inaccurate and uncertain. We confess that we cannot see any ground for this objection. However, to obtain a measurement of the time required, we arranged an electric contact mechanism, and recorded the time-intervals upon a kymograph. The average duration of the interval (4 series,—each of ten individual tests,—made at different stages during the progress of the experiment) was $1.02 \pm .06$ seconds.

was magnified into a noticeable lateral swing of the screen-edge. The difficulty was overcome by building a second track at the top of the screen. Great care was taken in planning the tracks; the result was a movement of the screen without perceptible tremor and without a trace of noise.

iii. *Results.* The experiments whose results are given below covered a period of time extending from November, 1901, until June, 1902. Each observer gave two one-hour sittings a week throughout that period. The observers were: Dr. I. M. Bentley (*B*), and four graduate students in psychology, Miss B. M. Downes (*D*), Messrs. R. H. Gault, (*G*), M. S. Macdonald (*M*), and H. C. Stevens (*S*).

The apparatus stood in a corner of the dark-room, where it was hidden from view by a curtain. None of the observers—excepting *B*—had any definite knowledge, before the experiments began, of the size or form of the apparatus. Not until the observer had gone into his dark corner and seated himself before the observation-tube was the curtain removed. It was always replaced again before he left his seat at the close of the sitting. So that no observer—with the single exception of *B*—had any extraneous aid in forming his judgments of the distances with which the experiments were concerned.

Observers *G*, *M* and *S* are emmetropic; *D* is hypermetropic (+ 1.18 D.). The distance of the near-point of accommodation of the eyes employed in the monocular experiments were: *B*, 143 mm., *D*, 186 mm., *G*, 132 mm., *M*, 118 mm., and *S*, 123 mm.; no spectacles were worn in any of the experiments. *B*'s eyes are defective in that they possess what may perhaps best be described as an unusually great inertia of accommodation. He finds that after accommodating continuously on a near object, he is unable for some time to get a clear image of distant objects. Thus, after reading for an hour or two, he is able only after the lapse of fifteen or twenty minutes to recognize a friend on the opposite side of the street. Whether the difficulty is due to a muscular or lenticular (*i. e.*, decreased plasticity) defect we cannot say. Unfortunately no oculist within reach has been able to diagnose the case, or to make a quantitative determination of the defect. We hope later on to be able to give a more exact characterization of the case.

The experiments fall into five groups: (A) Monocular experiments, in which the change of distance was made abruptly; (B) Binocular experiments in which the distance changed abruptly; (C) Monocular experiments with gradual change of distance; (D) Monocular and (E) Binocular estimations of absolute distance.

Our chief concern will be with the results of group (A). The binocular experiments were made solely for purposes of comparison.

(A.) MONOCULAR ABRUPT SERIES.

The object of these experiments was to determine the limens of approach and recession (as defined below) for five standard distances—286 mm. (3.5 D.), 333 mm. (3 D.), 400 mm. (2.5 D.), 500 mm. (2 D.), and 667 mm. (1.5 D.). We employed a serial method, without knowledge, working from equality in both directions. At the beginning of each series, the two screens, L_1 and L_2 , were set at any one of the standard distances. Then the shutter at the end of the observation-tube was opened; the observer fixated the edge of L_1 until perfect definition resulted, gave the signal for the movement of the frame EFGH which should swing in the second screen L_2 , and then fixated the edge of the latter. He gave his judgment as to the relative distance of L_1 and L_2 , and his introspections. For the next exposure, L_2 was set at a different distance from the observer, nearer or farther, and the two screens were successively exposed as before. The series reached its natural conclusion when the observer was sure of the direction in which the second screen had been moved along the slide N_2 . The change of distance in each successive setting of the second screen varied from five mm. to ten mm., according to the absolute distance of the standard screen. It remained constant throughout a series.

The nature and conditions of the experiment demand that each series shall proceed from equality to inequality, and never in the opposite direction. An illustration will make this evident. If in the initial exposure of any series the second distance were considerably greater or considerably less than the first, the second screen-edge would appear in dispersion images. Dispersion images would then furnish a criterion of inequality of distance; and so often as they recurred, the observer would be able to say without hesitation that the two distances were unequal. If, now,—as the procedure assumes,—he knew the relative positions of the screens in the initial exposure of the series, and knew also that the series were continuous in one direction, the criterion furnished by dispersion-images would warrant a judgment as to relative distance until equality were almost reached, *i. e.*, until the dispersion-images vanished from the series. Hence in an experimental series proceeding from inequality, judgments of relative distance would be possible without recourse to changes of accommodation and convergence.

We have, accordingly, in our experiments, proceeded only from equality. It is impossible that dispersion images could have been a contributing factor in the results tabulated below. For while dispersion images constitute a criterion of inequality of distance, they are powerless, at least under the experimental

conditions of the present investigation, to furnish evidence of the direction of inequality. They arise when the second point of fixation is nearer *or* farther than the first; they are consequently incapable of prompting a judgment that the second is definitely nearer, or is definitely farther than the first.

Each series was continued until the observer had reached a considerable degree of confidence in his judgment of nearer or farther. For the sake of convenience, symbols were employed to denote five increasing degrees of confidence. Confidence 1 denoted the minimal degree; confidence 5 denoted absolute certainty. Three approximately equal stages intermediate between these extremes were differentiated, and designated by the symbols 2, 3 and 4.

About ten determinations of each limen were made. In order that time might be saved, the series was seldom continued after confidence 3 had been reached.

(B.) BINOCULAR ABRUPT SERIES.

For this series a binocular observation-tube was fitted to the apparatus in place of the monocular tube. The observer now used both eyes in fixating the screen edge. Otherwise, the apparatus and conduct of the experiment remained unchanged.

(C.) MONOCULAR GRADUAL SERIES.

A modified form of apparatus was employed in these experiments. The frame EFGH was removed; and a smooth hardwood track was made fast to the top of the table at the place indicated by the dotted line T in the diagram. Upon this track ran a carriage provided with grooved wheels. One of the screens used in the previous experiments was fastened to the carriage. About 75 cm. above the surface of the table was fitted a second wooden track parallel with the first; the screen carried on its upper edge a wheel which ran on this upper track. This device made it possible to move the screen to and fro without noise and without tremor. A metal pointer, fixed to the carriage, just cleared a millimeter scale, and indicated the distance of the screen from the observer's eye. The method of conducting the experiment was as follows. The shutter was opened, and the observer was asked to fixate the edge of the screen. When he had obtained a perfect image of the edge, he gave a signal; the screen was now pushed with a constant and uniform motion along the track. The rate of motion employed for all points of the scale was seven centimeters in ten seconds. The observer gave a signal when he first detected movement, and a second signal when he was able to detect the direction of movement.

TABLE I.
*Results of Monocular Abrupt Experiments. a. Confidence 1. Showing the points at which Approach and
 Recession of the Second Screen were first noticed.*

286 mm.		333 mm.		400 mm.		500 mm.		667 mm.			
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	
Settings of Second Screen.	<i>G.</i>	270±2	320±13	318±1.6	384±9	375±4	458±7	474±5	563±8	618±17	750±19
	<i>M.</i>	260±3	318±8	305±6	373±6	357±5	458±15	450±7	566±14	593±13	760±21
	<i>S.</i>	258±6	325±9	305±5	376±14	360±8	456±15	446±10	565±10	596±15	747±24
	<i>D.</i>	255±5	334±11	288±6	403±16	352±6	506±26	444±13	604±21	578±16	798±34
Limens ¹ in % of Standard Distance.	<i>G.</i>	5.6%	12%	4.5%	15.3%	6.3%	14.5%	5.2%	12.6%	7.4%	12.5%
	<i>M.</i>	9.1	11.3	8.4	13	10.6	14.5	10	13.2	11.1	14
	<i>S.</i>	9.7	13.7	8.4	12.8	10	14	10.8	13	10.7	12
	<i>D.</i>	10.9	16.8	13.5	21	12	26.5	11.2	21	13.4	19.7

¹By 'limens,' as explained above, we here mean the difference in distance between the standard and variable screens at which the observer first passed judgment of 'nearer' or 'farther' with a given degree of confidence.

TABLE II.

Results of the Monocular Abrupt Series. b. Confidence 3. Showing the point at which the Approach or Recession of the Second Screen was observed with a fair degree of confidence.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	252±7	365±12	302.6±8.3	430±32	355±16	512±26	446±17	625±31	602±12.7	847±21
M.	237±7.6	342±6	288±8	399±11	330±6	492±18.4	403±9	607.5±22	550±25	840±45
S.	247±6.6	340±10	287±4.3	402±7.4	340±8	487±11	424±8	610±32	569±18	787±49
D.	230±6.6	376±8.8	278±10	442±35	332±8.3	554±12	432±25	688±42	532±41	880±35
G.	11.9%	27.7%	9.1%	29.1%	11.3%	28%	10.8%	25%	9.8%	27%
M.	17.2	19.6	13.5	19.8	17.5	23	19.4	21.5	17.6	26
S.	13.7	18.9	13.8	20.7	15	21.8	15.2	22	14.7	19.1
D.	19.6	31.5	16.5	32.7	17	38.5	17.4	37.6	20.3	32

TABLE III.

Results of the Binocular Abrupt Series. Confidence 1. Showing the points at which Approach or Recession of the Second Screen was just noticed.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	282±1.5	291.6±1	330±0	337±.8	395±0	405.7±.9	492.5±2.5	508.3±2.2	655±2.5	676.6±2.3
M.	283±0	290±2	327±1.3	340±1.5	393±1	405±0	495±1.7	507.5±2.3	656±3	679±2.8
S.	281±1.3	291±1	327±0	339±1.8	395±1.3	407±2	490±0	507±1.8	650±1.7	673±2.7
D.	277±1.5	293±2	325±.9	342.5±2.5	389±2.4	409±1.5	487.5±3	510±0	647±4	683±3
B.	280±2.3	293±1.7	328.3±2.3	338±2	392.3±1.5	410±0	493±1.3	510±0	653±2.2	680±3.3

G.	1.4%	1.96%	.9%	1.2%	1.3%	1.4%	1.5%	1.7%	1.8%	1.4%
M.	1.1	1.4	1.8	2.1	1.8	1.3	1	1.5	1.7	1.8
S.	1.8	1.8	1.8	1.8	1.3	1.8	2	1.4	1.7	1.8
D.	3.1	2.5	2.4	2.9	2.8	2.3	2.5	2	3	2.4
B.	2.1	2.5	1.5	1.5	1.9	2.5	1.4	2	2.1	1.9

¹The comparative irregularity of these binocular limens is doubtless due to the relatively small number of binocular determinations.

We give below, in separate Tables, the average limens of movement, and of direction.

TABLE IV.
Monocular Gradual Movement. Showing the points at which movement was first noticed in Approach and in Recession.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach. Recession.		Approach. Recession.		Approach. Recession.		Approach. Recession.		Approach. Recession.	
G.	241	375	287	425	344	493	417	601	578	778
M.	220	365	271	420	320	515	410	622	550	807
S.	232	388	280	479	325	570	396	664	550	850
D.	225	380	289	443	328	533	415	620	552	805
B.	240	421	278	507	325	610	405	706	555	927
G.	15.8%	31.2%	13.8%	27.6%	14%	23%	16.6%	20%	13.4%	16.7%
M.	23	27.7	18.6	26	20	29	18	24	17.6	21
S.	18.9	35.7	16	44	19	43	21	33	17.6	27.5
D.	21	33	13	31	18	33	17	24	17.3	21
B.	16	47	16.5	52	19	52	19	41	16.8	39

TABLE V.
Results of Monocular Gradual Series. Direction discerned. Confidence 1. Showing points at which direction of gradual movement was first observed in Approach and in Recession.

	286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
G.	229	415	272	462	323	515	388	620	520	827
M.	212	398	247	469	285	543	366	656	496	848
S.	219	407	251	516	313	596	374	679	511	873
D.	201	513	243	539	284	684	343	770	500	892
B.	183	875	196	683	197	750	234	883	242	—
G.	20%	45%	18%	39%	19%	29%	22%	24%	22%	24%
M.	26	39	26	41	29	36	27	31	26	27
S.	23	42	25	55	22	49	25	36	23	31
D.	30	79	27	61	29	71	31	54	25	34
B.	36	206	41	105	51	88	53	77	64	—

(D.) MONOCULAR AND (E.) BINOCULAR ESTIMATION OF
ABSOLUTE DISTANCE.

The form of apparatus described under (C) was employed in the experiments upon monocular and binocular estimation of absolute distance. The screen L_1 was set at any one of the distances to be estimated (286 mm. up to 667 mm., and 900 mm.); the observer fixated the edge, and estimated its distance in inches.

TABLE VI.
Estimates of Absolute Distance—Monocular.

	286 mm.	333 mm.	400 mm.	500 mm.	667 mm.	900 mm.
G.	185 \pm 33	236 \pm 41	342 \pm 52	386 \pm 33	546 \pm 68	603 \pm 25
M.	183 \pm 28	249 \pm 43	354 \pm 42	412 \pm 46	569 \pm 43	656 \pm 41
S.	188 \pm 46	285 \pm 66	345 \pm 38	452 \pm 25	610 \pm 33	823 \pm 39
D.	163 \pm 31	189 \pm 34	234 \pm 28	343 \pm 28	439 \pm 51	585 \pm 43
B.	407 \pm 72	367 \pm 39	325 \pm 46	372 \pm 76	467 \pm 82	435 \pm 37

Relation of Estimated to Actual Distance—Expressed in Per Cents.

	65%	71%	86%	77%	82%	67%
G.	65%	71%	86%	77%	82%	67%
M.	64	74	89	82	85	73
S.	66	86	86	90	92	92
D.	57	56	59	69	66	65
B.	142	110	81	74	70	48

III. DISCUSSION OF RESULTS.

(A.) MONOCULAR ABRUPT SERIES.

Several features stand out prominently in the results of these experiments. 1. The differential limens of approach bear an approximately constant relation to the stimulus distances. 2. The limens of recession are also fairly regular throughout the series. 3. The limens of approach are uniformly less than those of recession. 4. In all cases the limens are less than one would expect from Hillebrand's investigation with the same apparatus. 5. The capacity to sense differences of distance shows a well-marked individual variation. 6. The smallness of the mean

variation indicates that all four observers estimated changes of distance with a high degree of definiteness.

TABLE VII.
*Estimates of Absolute Distance—Binocular.**

	286 mm.	333 mm.	400 mm.	500 mm.	667 mm.	900 mm.
G.	198 ± 28	279 ± 25	366 ± 41	432 ± 35	513 ± 51	653 ± 48
S.	185 ± 15	233 ± 33	300 ± 38	422 ± 32	630 ± 59	884 ± 48
D.	252 ± 23	300 ± 25	348 ± 11	429 ± 51	493 ± 36	744 ± 28
B.	254 ± 11	310 ± 18	346 ± 33	422 ± 77	539 ± 58	713 ± 84

Estimated Distance in per cent. of Actual Distance.

	69%	84%	92%	86%	77%	73%
G.	69%	84%	92%	86%	77%	73%
S.	65	70	75	84	95	98
D.	88	90	87	86	74	83
B.	89	93	87	84	81	79

*Through an oversight, *M's* series of binocular estimates was left incomplete. Hence his name does not appear in this Table.

None of these features is peculiar to our investigation alone. Wundt's original thread experiments¹ give at least a hint of the regular increase of limen with increase of distance; while in Arrer's results² the regularity of increase is but little less pronounced than in our own. Wundt's early experiments brought to light the fact that approach is more readily perceived than recession; this observation was confirmed in the main by Arrer's results, though exceptions were found also by Hillebrand and Dixon. Hillebrand's investigation points to an unusually large differential limen;³ indeed, a difference of half a diopter seems to be the smallest limen determined for any of his observers. Yet Dixon, under identical experimental conditions, found 77% of right judgments when the screens were separated by only .05 diopter difference.⁴ In Wundt's experiments the limens varied between 4% and 11% of the standard distance; Arrer, also employing a thread for fixation-

¹ *Beiträge*, p. 114.

² *Phil. Studien*, XIII, pp. 139-142.

³ *Zeitschrift*, VII, pp. 126-9.

⁴ *Mind*, N. S., IV, p. 210.

object, obtained limens as low as 1.6%. The maximal and minimal variations determined by Arrer¹ indicate that the mean variation of his observers was even less than that of ours. Individual variations are common to the results of Hillebrand, Dixon and Arrer.

The judgments of one of our observers—*B*—are omitted from our tabulated results. It was discovered early in the course of the experimentation that *B* differed essentially from the other observers, not only in degree of readiness and correctness of judgment, but also, to all appearances, in the nature both of the criterion and the conscious process involved. *B*, *G*, *M*, and *S* almost invariably gave their judgments an instant after the appearance of the second screen; it was found that if they hesitated, even for only a few seconds, they were lost. They seldom made an error; almost never did they give a wrong judgment with any considerable degree of confidence. In 86 determinations *G* had only 5 wrong series of judgments which reached Confidence 3; *D* in a total of 83 had 4 wrong; *M* and *S* had none wrong in totals of 93 and 88 respectively. *B*'s judgments never came instantaneously; after the exposure of the second screen, there always elapsed a period of hesitation during which *B* was evidently deliberating and groping about for a clue. His judgments were usually given in such terms as: "It must be farther" (referring of course to the second screen); "It is less clear-cut; I should say nearer;" "It is more distinct; I think it is farther." These judgments were never characterized by the immediacy which seemed to be a necessary condition in the case of the other observers. Attempts were frequently made to draw a judgment from *B* soon after the second screen appeared, but always without success. If the second screen came into view with its edge blurred, the indistinctness persisted for some seconds, and frequently refused to disappear even after a tedious delay. It was found to be impossible to get the full complement of determinations from *B* although he gave more sittings than any other observer. Twenty-five single judgments in the hour was a limit beyond which he could not go. He usually suffered from fatigue in the eye, towards the close of the sitting; *D*, *G*, *M* and *S* averaged 80 or 90 judgments in the hour. While they rarely made an erroneous judgment, *B* seldom carried a set through without error. Of his 53 series, 26 were absolutely wrong, even when Confidence 3 was reached.

There can be no doubt that *B*'s criterion was wholly different from that employed by the other observers. The immediacy of their judgments is in striking contrast with his delib-

¹ *L. c.*, p. 142.

eration; his inaccuracy is the more surprising in view of their precision. Since his accommodation failed to change on the appearance of the second screen, or changed so slowly and uncertainly as to be attended by no definite sensible effects, he was compelled to have recourse to another criterion. He seems to have hit upon a clue in dispersion-images, and to have inferred nearness from indistinctness. If, on the other hand, the edges seemed approximately equally clear-cut, the second screen was usually judged farther, even when the two were equi-distant. He frequently reported that the screens were unequally dark, and judged the darker screen to be nearer. It is extremely doubtful if there was any difference in the brightness of the two screens. Care had been taken to make them as nearly identical in appearance as possible; no other observer discovered any inequality of illumination, and *B*'s introspections in the binocular experiments make no mention of unequal brightness.

Here are the records of a few typical series:

Obs. <i>B.</i> Feb. 22, 1902.		10 A. M.		Condition normal.	
DISTANCE OF STANDARD SCREEN.		DISTANCE OF VARIABLE SCREEN.		JUDGMENT.	CONFIDENCE.
667 mm. (left screen)		667 mm. (right screen)		{ Second screen darker. Seems nearer. }	2
667 (right)		667 (left)		{ Second lighter. Must be farther. }	1
667 (left)		667 (right)		{ Second lighter. Seems farther. }	2
667 (right)		667 (left)		Equidistant.	
Obs. <i>B.</i> Jan. 24.		10 A. M.		Condition normal.	
500—500		Equal.			
"—up to 590		Equal.			
"—600		Second less distinct.	Nearer.		2.
"—608		" " "	"		2.
"—616		Second less distinct but lighter.	Farther.		1.
"—624		Second less distinct.	Nearer.		2.
"—632		Second less distinct but lighter.	Farther.		1.
"—640		" " "	"	Farther.	2.
"—648		Second less distinct.	Nearer.		2.
"—656		" " "	and darker.	Nearer.	2.
"—664		" " "	" "	"	3.
Obs. <i>B.</i> March 8.		11 A. M.		Normal.	
333—333		Equal.			
333—up to 375		"			
333—380		Second nearer.	Cannot say why.		1.
333—390		" "	Darker.		2.
333—400		" "	less distinct.		2.
333—410		" "	" "		2.
333—420		" "	darker.		2.
333—430		" "	less distinct.		2.
333—440		" "	" "		3.

sensation seemed to furnish the basis for his judgments throughout the whole investigation. Only in rare cases did *D*, *M* and *S* report a sensation of strain. They usually failed to find any basis for their estimation. "It just seems nearer (farther); that is all." *G*'s judgments, however, were not a whit less immediate than those of *D*, *M* and *S*. It was only when he came to introspect, that he mentioned the sensation of strain. Care had been taken to avoid the suggestion of this sensation to any observer. Indeed, when we seemed to discredit the presence of *G*'s sensation of strain, he insisted the more strongly that he could not be mistaken in it. It is worth noting that the only observer who was able to discriminate between his positions of accommodation by a distinct consciousness of strain, has much lower limens of approach than the others. (See pp. 178 ff.)

Obs.	<i>S</i> .	Feb. 15.	3 P. M.	Normal.
		500—500	Equal.	
		500—up to 558	"	
		500—564	Slight difference. Second perhaps farther.	1.
		500—572	Second came in farther.	1.
		500—580	" " " "	2.
		500—588	" " " "	2.
		500—596	" " " "	2.
		500—605	" " " "	3.
Obs.	<i>D</i> .	March 13.	2 P. M.	Normal.
		333—333	Equal. "Both horribly near."	
		333—up to 400	Equal. "Very unpleasant."	
		333—405	Second seems farther.	1.
		333—410	" " " "	1.
		333—415	Farther. Simply comes in at greater distance.	2.
		333—420	" " " " " "	2.
		333—425	" " " " " "	2.
		333—430	" " " " " "	2.
		333—435	" " " " " "	3.

These specimen records show the general character of the two types of judgments and introspection. The one type is characterized by its immediacy; the most searching scrutiny failed—save in the case of *G* alone—to find any introspective evidence of the presence in consciousness of a sensation-basis for the judgment. *G*'s estimations were no less direct in character, but introspection almost invariably revealed the presence of a sensation factor which made the judgment possible. The other type of judgment is anything but immediate in character. Not only was the time interval between the exposure and the judgment invariably longer, but introspection discovered conscious processes, not muscular but retinal, by which the judgment was finally mediated. Moreover, as has already been pointed out, the types are as divergent in accuracy as they are in character. We believe that we are justified in assuming

that the judgments of persons possessed of the normal power of accommodation, belong to the former type.

An attempt was made, towards the close of the series of monocular abrupt experiments, to determine how far a 'conscious impulse of will,' as assumed by Hillebrand, could play a part in the judgments of relative distance. The observers were directed to make a voluntary change of accommodation from near to far or from far to near, when the second screen appeared, and to estimate its relative distance in the light of what transpired. The results invariably tell against Hillebrand's assumption. It was usually discovered that the observers were unable to change their accommodation at will in the sense assumed by Hillebrand. It was found, however, that they could see the second screen distinctly by simply willing to see it distinctly. But *such an act of will furnished no conscious datum of central origin which could give a clue as to the direction in which the change of accommodation had been made.* In some few cases the appropriate change of accommodation was brought about voluntarily, but this procedure proved to be a hindrance rather than a help to the accurate estimation of relative distance. In short, the unanimous verdict of all four normal observers goes to show that if the judgment is not made immediately it cannot be made at all.

Whether the cultivation of such an artificial means of estimating distances as Hillebrand describes could yield good results, even in the hands of observers trained to a voluntary control of their accommodation mechanism, is exceedingly problematical. Except that he gives it a name, Hillebrand leaves us absolutely in the dark as to the basis of the judgment. From what source does the conscious datum come which serves to determine what our judgment in a given case shall be? Whence do we get the raw material which enables us to differentiate, to judge now nearer, now farther? If, with Hillebrand, we deny that peripheral sensations are aroused in the adjustment of the ocular mechanism, we find ourselves compelled either to deny the possibility of estimating space monocularly, or to resurrect the deceased theory of innervation. To choose the former alternative means to dispute the results of Wundt, Dixon, Arrer and Hillebrand himself. To choose the latter, means to maintain that the voluntary initiation of movement is attended by a sensation of central origin, whose intensity is gauged by the amount of energy put forth into the movement. This in turn means to ignore the results of several decades of physiological and psychological research, and to take one's stand upon a theory long since discarded.

The results of the twenty-seven correct determinations made

by *B* are appended—not for comparison, because they are for obvious reasons incomparable with the results of the other observers, but solely in the interests of completeness.

TABLE VIII.
Results of Monocular Abrupt Experiments. a. Confidence 1. Showing averages of B's Correct Determinations.

286 mm.		333 mm.		400 mm.		500 mm.		667 mm.	
Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.	Approach.	Recession.
250±4	420±12	297±5	407±39	366±9	500±17	473±4	640±13	610±12	775±25
12.6%	47%	10.8%	22%	8.5%	25%	5.4%	35%	8.6%	16.2%
<i>b. Confidence 3.</i>									
235±3	560±0	285±15	457±29	323±18	520±26	450±20	720±31	523±45	865±0
17.9%	96%	14.4%	37.2%	19.3%	30%	10%	44%	21.3%	29.7%

(B.) BINOCULAR ABRUPT SERIES.

The principal features of these results are: 1. The liminal values are extremely small. 2. There is a large individual variation among the observers. 3. The recession limens are in general greater than those of approach—although in about 40% of the determinations they are approximately equal. 4. The proportion of limen to stimulus is approximately constant both in approach and in recession. 5. The most striking feature of Table III is, however, in connection with *B*'s results. *B*'s monocular judgments of relative distance were a failure; his binocular judgments compare favorably with those of the other observers. 6. It is to be noted, too, that *G*'s monocular limens—particularly those of approach—were much smaller than those of any other observer; whereas his binocular results do not differ materially from the average determinations.

The introspections of this series show the presence here of but a single type of judgment—and that of the immediate character already described. *B*'s introspections differ in no essential particular from those of the other observers. He reports that "the second screen came in at a greater distance," or that "it simply stands out in front," etc. In comparatively few instances did an observer succeed in discovering the factor on which he based his judgment; it was described as a sensation of strain in the two eyes—and it occurred chiefly with near distances. The localization of this sensation was indefinite; no observer was able to state that its origin was in the external muscles of the eyeball and not in the ciliary muscle.

As in the monocular experiments, so here it was found that the judgment must be given without delay. If hesitation arises, doubt ensues and estimation becomes impossible. An introspection of *B*'s reads as follows (screens at 500 mm. and 492 mm.): "The second screen stands out nearer to me. No difference in clearness; no noticeable difference in strain," and at another time: "the second simply came in nearer. After a moment I could no longer be sure that they had been different." Apparent recession on continued fixation of a near screen was remarked by all observers.

(C.) MONOCULAR GRADUAL EXPERIMENTS.

These results show that: 1. Movement was almost invariably perceived before direction was discerned. 2. Limens both of approach and of recession are higher with gradual than with abrupt change of distance. 3. Limens of approach are uniformly lower than those of recession when the change of distance is gradual. 4. *B*'s limens are in every case higher than those of the other observers; indeed *B* detected no recession be-

tween the limits of 667 mm. and 950 mm. (the extreme limit of the apparatus); but 5. *B*'s errors are comparatively insignificant in the gradual experiments. In a total of 90 series he had but 9 errors, 8 of which were failures to detect recession of the screen.

The introspections in this series bring to light the same two types of judgment as were found in the monocular abrupt experiments. Here, as formerly, *D*, *M* and *S* gave immediate judgments for which no evidence could be found in consciousness. *M* failed in all cases to describe the conscious process which led to his judgment. *S* reported, over and over again: "I felt it go away (come nearer). No blur; no strain." *D* was frequently startled in the approach experiments by the suddenness with which "it seems to rush up toward me." *G* usually reported perceptible increase or decrease of strain. On the other hand, *B* seemed in all cases to infer the direction of motion from the rate of blurring of the screen-edge; if the blurring increased rapidly, he inferred approach; if slowly, recession.

It was suspected that the other observers were unconsciously employing the same criterion. But the results obtained from a modification of the experiment showed that this suspicion was groundless. Series were occasionally introduced, in which the screen was made to approach with extreme slowness, and to recede with corresponding rapidity. This change of method was followed by no change in the accuracy of the judgments of *D*, *G*, *M* and *S*, but it invariably led to an error in *B*'s estimation. It is safe to conclude that all of *B*'s experiments with gradual change of distance are failures, so far as the muscular sensations of accommodation are concerned.

Hillebrand's observers were unable to estimate gradual changes of distance with any degree of accuracy. They usually refused even to offer a conjecture as to the direction of the movement; and when they did make a judgment, they were as often wrong as right. It is to be noted that Hillebrand's results are really incomparable with ours on account of the difference of experimental conditions. Hillebrand's movement began before the shutter was opened, and hence may be assumed to have been well under way before the observer secured his first accommodation upon it. We repeated a few of his experiments; and while we did, in some few cases, obtain positive results, still the judgments were vague and indefinite and frequently erroneous. His method was finally abandoned, for two reasons. 1. If, when, the fixation-object first comes into view, it is at once near to the observer's eye, and moving directly towards or from him, accommodation upon it must necessarily be difficult and uncertain. Hence the negative

character of the results of experiments with such a fixation-object does not justify the conclusion that accommodation *never* contributes to the perception of distance. It can only be said that experiments of this type are from their very nature inadequate to a positive solution of the problem in hand. 2. The method would be inaccurate and indefensible even if it yielded results of a positive character. For if the screen be in motion before the observer accommodates upon it, we have no means of determining its precise position at the instant when he first obtains a clear image. Hence when the judgment of 'nearer' or 'farther' is finally given, we still have no record of the initial position to which it refers. It is obviously impossible to determine the relation of two positions of accommodation, one of which is, and must remain, unknown. Hence the results of such experiments cannot contribute to a solution—positive or negative—of the problem.

It would be interesting to learn how Hillebrand would reconcile the negative results of these experiments with his assumption of a 'conscious impulse of will,' by which changes of accommodation are accomplished, and mediated to consciousness. Changes of accommodation must have come about, if the moving screen were seen in perfect definition when it stood at different distances. If, now, these changes were the product of a '*conscious* impulse of will,' how did it happen that the observer was unconscious of change of distance?

Hillebrand's experiments with the Aubert diaphragm are also said by their author to tell against the participation of sensations of accommodation in the perception of depth. He found that when the diaphragm was slowly advanced, its aperture meanwhile being rapidly decreased, there resulted a judgment of recession, notwithstanding the greater tension of accommodation. Here, again, his demonstration is as fatal to his own theory as it is to that of Wundt.

This experiment, however, is irrelevant to the question: we are not concerned with the determination of the relative significance of the various criteria of distance. It fails to disprove that sensations of accommodation may contribute to the perception of depth. That these sensations constitute the sole—or even the most effective—factor of depth-localization has never been maintained. Decrease of visual angle was obviously the determining factor in this experiment. It is not incomprehensible that a factor which is effective under favorable circumstances may be swamped and rendered inoperative under adverse conditions. From the point of view of accommodation, the change of visual angle was a distracting element of superior power. That the stars cannot be seen at noon is no proof that they are forever invisible.

D and *E*. THE ESTIMATION OF ABSOLUTE DISTANCE.

The method of recording the estimates in our experiments was not free from objection. The estimate was expressed in terms of inches; it resulted from the comparison of the perceived stimulus distance with a distance mentally reproduced. For this reason, one must be cautious in drawing conclusions from our tabulated results, which make a direct comparison of the recorded estimates with their objective stimuli.

Whether or not these results may be accepted as a corroboration of the principle that distance is ordinarily under-estimated in the absence of secondary criteria, they at least establish the fact that absolute distance was estimated with some degree of definiteness by our observers.

It has been asserted by Wundt¹ and repeated by Hering² that under conditions such as were present in our experiments, the absolute distance of the fixation-object cannot be estimated. This statement is not in accord with our results. Even at their first sitting, our observers were able to express their judgments in absolute terms. In the initial experiments they were asked to estimate, not the relative position of the second screen, but the absolute distances of both screens. This they seldom failed to do; page after page of our earlier records of the monocular abrupt experiments is filled with such results as these:

<i>Stimulus distances.</i>	<i>Estimates. (Recorded in inches but here expressed in mm.)</i>
333 mm.—500 mm.	229 mm. and 305 mm.
667 " —333 "	457 " " 254 "
500 " —250 "	305 " " 127 "
667 " —667 "	381 " " 381 "
333 " —250 "	254 " " 178 "
333 " —310 "	254 " " 254 minus mm.

The possibility of absolute localization cannot be denied. Our results can teach us little, however, of the capacity to localize definitely (with small mean variation) and correctly (in agreement with reality).

IV. INTERPRETATION AND CONCLUSIONS.

The object of this investigation is to determine the influence of accommodation and convergence upon the perception of depth. Our experimental results have already pointed to a positive solution of the problem.

The investigation has shown that under experimental condi-

¹*Beiträge*, p. 107.

²*Raumsinn*, pp. 546-7.

tions which exclude all known criteria of depth—save only accommodation and convergence—it is still possible to perceive changes of distance. This establishes the fact that either accommodation, or convergence, or both contribute to the perception of depth.

It has shown, too, that the absence from monocular vision of normal sensitivity to relative distance is not necessarily attended by a corresponding abnormality in binocular vision. This demonstration warrants the conclusion that the factors which determine the judgment of relative distance are essentially different in monocular and in binocular vision. This conclusion is supported by the fact that the limens of approach and recession bear an essentially different relation to the stimulus-distances in the two cases.

It has also been shown that in monocular vision—when the factors present are reduced to accommodation and convergence—the presence of normal accommodation has been attended by the capacity accurately to estimate relative distances, while the absence of normal accommodation has been paralleled by an absence of this capacity. From this we conclude that *accommodation constitutes the essential criterion of depth in our monocular experiments.*

It follows from our binocular results that the essential criterion in the binocular experiments was not furnished by accommodation.

The hypothesis that increased tension of accommodation gives rise to judgments of approach is not new in the literature. This theory was advanced by Berkeley, and has never since his time been without supporters. That the judgment of recession can be prompted by the opposite change of accommodation has never been generally maintained. Indeed Wundt explicitly asserted in his original paper¹ that this position is untenable. He held that since the change of accommodation which focuses a receding object at successive distances is accomplished by the relaxation of the ciliary muscle, it cannot give rise to sensation, and hence cannot constitute the basis of a judgment. This position is the logical consequence of the theory of innervation-feelings which Wundt then advocated.

If however it be held, as is now all but universally believed, that we become conscious of change of muscular tension, not through the medium of feelings of innervation, but through concomitant peripheral sensations, there is no ground for denying that the relaxation of muscle is attended by sensible effects.

¹ *Beiträge*, p. 110.

Muscle and tendon are richly supplied with sense-organs. It is conceded that the different terms of an ascending series of muscular contractions are discriminated. It is but reasonable, then, to suppose that the individual terms may still be differentiated, when the series recurs in descending order, from tension to relaxation,—*i. e.*, from greater to less degree of contraction. Hence, we are justified in concluding that accommodation may prompt judgments alike of farther and of nearer. This simply assumes that a degree of contraction of the ciliary muscle which is attended by muscular sensation if it follow upon a lesser degree, will be similarly attended if it follow upon a greater degree of contraction.

Histological investigation has shown that the organs of the muscular sensations are anatomically akin to the cutaneous organs of pressure. Von Frey's experiments¹ demonstrate—what, indeed, cannot well be disputed—that we are able to discriminate the successive stages not only of an ascending series of cutaneous pressure, but of a descending series as well. Moreover, von Frey is convinced that the differential limen is higher in the latter case than in the former.

If now this conception be carried over from the pressure-organs of the skin to the similar organs of muscle and tendon, one would expect to find that changes of accommodation arouse muscular sensations, whether they be accomplished by muscular 'contraction' (ascending series) or by muscular 'relaxation' (descending series of contractions). These two states of muscle are not different in kind; the latter is itself formulable in terms of contraction.

Since von Frey found that the differential limen of increase of serial pressure on the skin is lower than that of decrease, one would again expect to find that the same relation obtains between the differential limens of muscular 'contraction' and muscular 'relaxation.' In fact, the results of our monocular experiments prove that the limen of approach is invariably less than that of recession.

An attempt was made to photograph the images reflected by means of a phacoscope from the anterior and posterior surfaces of the lens when it was accommodated for each of the five standard distances and of the distances given in Table I as just noticeably nearer and farther. It was hoped by this means to determine the radii of curvature of the two surfaces of the lens in each of the fifteen positions of accommodation. From these data, assuming the volume of the lens to remain constant, the determination of the magnitude of its change of diameter for these various positions of accommodation seemed possible. The change of diameter would then indicate the liminal lengthening and shortening of the ciliary muscle. Several difficulties were encountered, however. It was found to be impossible, with any

¹ M. v. Frey: *Untersuchungen über die Sinnes-functionen der menschlichen Haut. Erste Abhandlung*, pp. 180-188.

available light intensity, to obtain a photograph of the lenticular images. The table of radii of curvature published by Weiss¹ furnished, with interpolations, the data for the anterior surface. Similar data for the posterior surface could not be obtained; though the changes of the latter surface are relatively slight, they can scarcely be ignored. Doubt as to the relative parts played by the straight and annular fibres of the ciliary muscle seemed likely to vitiate our results. Moreover, uncertainty as to the area of the lenticular surface affected by the change of curvature made accurate calculation impossible, and the plan was finally abandoned.

The essential features of our conclusions regarding the perception of relative distance in the monocular experiments may be summarized as follows:

(1) Both the approach and the recession of the fixation-object can be perceived.

(2) These changes of distance are perceived from the corresponding changes of accommodation.

(3) Muscular sensations of accommodation constitute the sense-basis of the perception both of approach and recession.

V. THEORY.

When we attempt to bring our results into relation with one or other of the modern theories of psychological space, we must, of course, recognize that these theories are much broader in scope than any single investigation can be. A theory of space must pronounce upon many questions which our problem leaves untouched. Yet it may with justice be maintained that the results of an experimental enquiry—however circumscribed its province—cannot but contribute to our knowledge of the general topic of which it forms a part. Hence it is probable that the conclusions reached in the present study may aid us in adjudicating the relative merits of the rival theories of space,—particularly since it has been concerned with the question upon which the rivals are most at variance.

It is in dealing with the phenomena of relative localization in binocular vision that Nativism has been most successful. The presence of a pair of retinas furnishes a natural basis for the assumption of a paired arrangement of retinal points. From this naturally follows the assumption that retinal disparity and double-images constitute the criteria of relative depth. The theory so far is plausible and alluring; its simplicity of conception and clear envisagement of the process of localization are strong points in its favor. Yet the ultimate criterion of the worth of any scientific theory must be its ability to account for all the facts which it seeks to systematize. And it cannot

¹O. Weiss: *Tabelle der zur Accommodation auf verschiedenen Entfernungen nöthigen Linsenwölbungen*. *Pflüger's Archiv*, XXVIII, 1902, p. 91.

be denied that however creditably Hering's theory may have acquitted itself as regards the binocular estimation of relative distance, its defects become apparent as soon as it passes beyond this narrow field. Its account of the binocular estimation of absolute distance, and its explanation of the phenomena both of absolute and of relative localization in monocular vision, must be rejected. Moreover, its failure to co-ordinate the phenomena of monocular, with those of binocular vision, casts suspicion upon the whole theory.

The Nativistic theory, whose conception of the process of binocular localization is outlined above, accounts only for the estimation of relative distance. The magnitude of retinal disparity is measured by reference to the retinal center; this central point of reference, is, in terms of the distance of the object imaged upon it, not a fixed but a variable quantity,—since the distance of the point of fixation changes with every change of convergence. Hence it follows that the disparity criterion can furnish only relative depth-values. These values are expressed in terms of the momentary position of convergence, and contribute to a localization relative to the momentary fixation-point. Further than this, a purely retinal theory cannot go. Attempts have been made, however, by Hering and by Hillebrand, to eke out this limitation by a supplementary hypothesis, which we shall later consider.

It is difficult to see how double-images and retinal disparity can furnish an unequivocal criterion of change of distance. Double-images arise, not only when the second fixation-point is nearer, but also when it is farther than the first,—the only difference being the presence of crossed disparity in the former, of uncrossed disparity in the latter case. Hence the assumption that double-images constitute an unequivocal criterion of change of distance, is tantamount to the assumption that crossed disparity can be distinguished from uncrossed, even when their magnitudes have diminished almost to the vanishing point. Experiments with Hering's falling ball apparatus have established a differential limen of about $\frac{1}{50}$. The difference of retinal disparity which is here assumed to be distinguished, approximates to an infinitesimal quantity. Yet Hering's theory asserts not only that these minute differences were detected, but also that there was in every case a consciousness of the kind of disparity present.

Laboratory experiments have frequently shown that observers of normal sensitivity to changes of depth have no immediate knowledge of the kind of disparity operative in a given instance, even when the double-images are manifestly present in considerable degree. The nature of the disparity can be discovered only indirectly, by closure of the one eye and observation of the surviving images with the other. We are not un-

mindful of the familiar phenomena of stereoscopic vision; but we fail to find that Hering's bold statement of the presence of disparity furnishes a satisfactory explanation of the facts in question. The tacit assumption of a retinal capacity, for which experiment gives no warrant, is an insecure foundation upon which to build a theory.

Attention has already been called to the fact that double-images were not consciously present in our binocular experiments. Moreover, it was frequently demonstrated that their intentional production rendered estimation impossible.

The apparent impossibility of distinguishing the double-images occasioned by a farther, from those occasioned by a nearer object, together with the inefficiency of double-images in our own experiments, leads us to reject Hering's criterion as inadequate to the task which it is called upon to perform.

Nor is this the only objection to Hering's conception. The impossibility of finding in the single retina a psychophysical substrate analogous to that posited as the essential condition of binocular localization, has necessitated the introduction of an extraneous depth-factor. Monocular localization in depth is held to be a function of the 'conscious impulse of will' through which changes of accommodation are made.¹ Monocular and binocular localization are thus differentiated, in that different sense-data and different mental processes are assumed in the two cases. Changes of distance are *seen* in binocular vision; in monocular vision they are in some way discerned through the mediating influence of will.

Here again Hering's assumption is given an unqualified denial by our results. The judgments of our observers followed no less immediately upon the exposure in the monocular than in the binocular experiments. No observer was able to discover a difference in kind between the mental processes involved. We grant, of course, that the levelling effect of time may have modified the processes in question. But that, even after the lapse of ages, two such essentially different processes should have become identical for introspection is scarcely possible. It seems incredible that observers trained in introspection should, in hundreds of trials, invariably fail to detect a difference between a purely sensational process, and a process which is anything but sensational in character.

A further objection to the will criterion assumed by Hering and Hillebrand, is the difficulty of giving it a definite con-

¹ Hering brings forward the principle of coincidence of attention with regard, to account for the localization of the point of fixation; but he identifies the function of attention with that of will (*Raum-sinn*, pp. 534 ff.). Hillebrand works out this concept in greater detail and posits a conscious impulse of will as the essential factor (*Zeitschrift*, VII, p. 147).

scious value. Unreasonable as it is to suppose that change of accommodation is ordinarily accomplished by voluntary movement, the matter becomes even more arbitrary when we have to assume that the amount and kind of impulse of will are given us in immediate experience. Just how we become conscious of these minute gradations of will-impulse remains a mystery. Muscular sensations of accommodation would furnish the most natural explanation; but that route is barred, inasmuch as it was the denial of the existence of such sensations that necessitated the impossible hypothesis of conscious impulses of will. Unless we concede the validity of the discarded theory of innervation-feelings, Hering is as far as ever from a satisfactory solution of the problem,—and this concession he can scarcely hope to obtain.

Our examination of Hering's theory has revealed the following defects:¹

1. It is based upon an assumed retinal capacity which does not exist.
2. Its appeal to the influence of will makes a term do duty for an explanation.
3. It makes an unnatural breach between the phenomena of monocular and of binocular vision.

Our conclusions agree in the main with those of Wundt and Arrer. Upon only a single point—the relative significance of accommodation and convergence in monocular vision—do we take issue with these writers. We have found reason to believe that accommodation is the determining factor in monocular vision, while convergence appears to gain the upper hand in binocular vision. The existence of sensations of accommodation and convergence, is an assumption without which our results cannot be explained. It is true that these sensations received uniform introspective confirmation only in the case of a single observer. But all observers bear occasional witness to their presence. And it is of the essence of Wundt's theory of psychological space as a synthetic function, that the sense-factors involved should, except under the most favorable experimental conditions of analysis, disappear from separate view.

¹ It may perhaps be of interest to learn that the writer began the experimental investigation with a decided leaning toward the theory of Hering. Its conception of the binocular arrangement of retinal points appealed to him as offering a satisfactory psychophysical substrate for the estimation of depth. Hillebrand's apparatus seemed, as it seems even yet, to offer the most advantageous features for an experimental investigation of the problem. Moreover, the criticisms of Wundt and of Arrer seemed, and still seem, to be in several cases unjust. The writer's change of view has been brought about chiefly by the results and introspections of his observers, but also by a more critical examination of the work published by Hering and Hillebrand.

STUDIES IN THE PSYCHOLOGY AND PHYSIOLOGY OF LEARNING.¹

By EDGAR JAMES SWIFT.

- I. ON TOSSING AND CATCHING BALLS.
- II. ON LEARNING SHORT-HAND.
- III. ON THE ACQUISITION AND CONTROL OF THE REFLEX
WINK.

Learning, though as various as human activity, may be roughly classed under one or the other of three types: The acquisition of skill, or learning to do; The formation of associations, or the acquisition of information; and the getting of control, or the formation of inhibitions. While it is doubtless true that it would be difficult, if not impossible, to find absolutely pure types of any of these, so fully is mental action of all sorts involved in each, yet for purposes of preliminary study the classification is adequate and each type is considered in the studies that follow.

I. ON TOSSING AND CATCHING BALLS.

The purpose of this study was to investigate the learning process, to inquire whether there is a typical curve of the acquisition of skill, and if so to determine its general form and so far as possible to find out what alters it in particular cases.

Method, apparatus and subjects. Keeping two balls going with one hand, receiving and throwing one while the other is in the air, was selected. There were several reasons for deciding upon this, some of which can be best appreciated by those who have tried to do it, but the chief argument in its favor was the accuracy that it permits in measuring the learner's progress.

The balls used were of solid rubber and weighed 122 6/10 and 130 2/10 grammes. This difference was not noticed by the subjects. Their diameters were 42 and 44 millimetres, respectively. Six subjects in all were tested; five with the regular series and one in keeping three balls up with two

¹The writer wishes to express his obligation to Dr. Edmund C. Sanford for his generous co-operation throughout this investigation. The President and other members of the faculty of Clark University have also shown interest, and the assistance of the librarian, Mr. Louis N. Wilson, in securing literature has been invaluable.

hands. Five of the subjects were university students and one was a professor.

The daily programme consisted of ten trials, the subject in each case continuing the throwing until he failed to catch one or both of the balls. The number of catches made in each trial was immediately recorded with any data obtainable as to the method pursued and the cause of failure. After each trial the subject rested as long as seemed necessary and then recommenced the throwing until the ten trials had been completed. There was no practice whatever between the tests and none of the subjects had ever handled balls in this way except as the base ball and tennis player may occasionally throw a ball or two into the air and catch them as they come down. All the subjects did the work in the afternoon. In the few instances in which a change of hour was necessary this fact was recorded. The total time occupied in the testing (and this testing constituted also the sole training of the subject) was various in amount extending from a few minutes in the early stages of practice to two or three hours toward the end. All the subjects knew their daily score and they always kept track of their progress during each test as well as from day to day. This method has undoubtedly given results different from those that would have been gotten had the subjects been kept in ignorance of the score, but the plan was uniform throughout and had the advantage of largely overcoming the effect of monotony which usually depresses those who are obliged to practice continuously for so many days. Besides, it enabled observation to be made, incidentally, on the effect of competition both with one's own record and with that of others.

The throwing and catching was chiefly with the right hand in each case (all the subjects were right-handed) but in order to test the effect of right-handed practice upon the skill of the left hand a preliminary test was made upon each subject, on the first day of his service, of his untutored skill with the left hand. This preliminary test consisted of ten trials as usual; and after it the left hand was not again tried until after the completion of the whole period of work with the right hand, when the left hand was again tested and a record of its progress kept for a number of days.

The daily training was continued in the case of four of the six subjects until their average number of catches per trial exceeded 100, or, what amounts to the same things, 1,000 catches in ten trials, upon two days in succession. In the case of the other two subjects the training was broken off at a lower score for reasons that will appear later.¹ The tests with the right

¹That even the lesser skill attained by these two was not bad seems evident when we find Hopkins saying that "the young man who can

hand were made every day, including Sundays, as were also those with the left, except as indicated below.

Still another phase of the experiment was an attempt to find how skill in the management of the balls declined after the cessation of daily practice. With this in view monthly tests have been made since the close of the regular experiments with the right hand, upon three of the subjects. The results of these "forgetting" tests will be given in their proper order below.

Influences that affected the score. It seems probable that the weight of the balls may have had an influence on the results on account of fatigue, and tennis balls would perhaps have sent the score up faster toward the end when the number of successive catches per trial at times exceeded two hundred. The essential course of the curve of progress would not, however, have been altered.

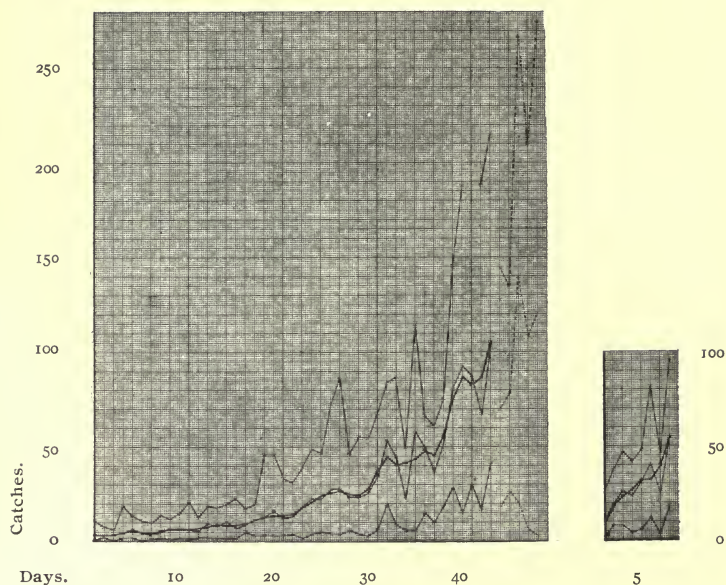
At the beginning the height of the room proved to be an important element, and the one in which the experiments were made was high enough to allow sufficient freedom in this particular.

The ball tossing proved itself an unexpectedly delicate index of bodily and other conditions. Slight changes in physical condition or in the temperature and illumination of the room often produced noticeable effects upon the score. This results in marked unevenness in the record from day to day, but does not influence unfavorably the general features of progress in which we are here most interested. Indeed, some of these disturbing elements are present at intervals in every learning process, and the importance of taking daily records of the progress in such work, instead of week by week, is apparent when we notice the great variations in skill through which most of the subjects passed.

The most frequent evidences of lack of skill in the earlier days of training were "wild throwing" (the tossing of the ball in such a way that it fell out of easy reach), and clumsy catching, *i. e.*, not being able to capture the ball when it touched the hand. As the subjects progressed somewhat, another source of failure appeared in the collision of the balls in the air, the ascending ball striking the descending one and knocking it out of reach. In the final stages trouble of this sort was again less frequent.

perform this operation twenty times without dropping one of the balls can treat the artist of the circus as a *confrère*." (Hopkins: Magic, p. 140.) Hopkins possibly means, however, a young man who *never* fails to reach at least twenty catches, however often he may try. If this is the case, he is speaking of a degree of skill which was hardly reached by my subjects.

CURVE A.



For all the Curves. The curve for the right hand is at the left. The upper, thin, line shows the highest single score (*i. e.*, the greatest number of catches made in a single trial before missing) for each day, while the lower, thin, line gives the opposite extreme, *i. e.*, the lowest single score for corresponding days. The thin line in the middle represents the daily average, and the heavy line that cuts across it is the smoothed average. The method of smoothing has been described above. (See page 210, foot note.)

The number of catches before missing is shown on the vertical axis and the days on the horizontal axis.

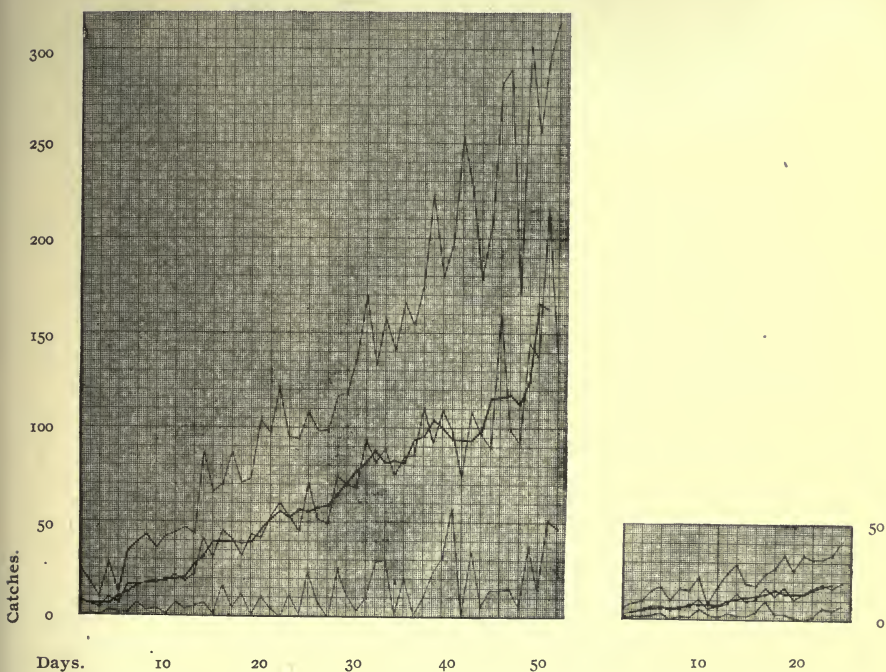
The "forgetting" curve is given in dotted lines. The three lines stand for the highest, average, and lowest respectively. Here the unit on the horizontal axis is thirty days. For the discussion of the results see page 221.

The curve for the left hand is at the right. The short line at the right of the vertical axis near its base and perpendicular to it shows the score made by the left hand before the right hand practice began. (See pp. 218-220.)

For Curve A. The break in the highest score, on the third day before the last, was caused by the subject being "called" every time at one hundred. He was trying to make ten scores of one hundred each.

Subject A played base ball when a boy but was only a mediocre player. He had never tried to keep two balls in the air until these experiments were begun. His method was to keep the balls in two parallel columns in front of him. He found himself doing this and then consciously continued it.

CURVE B.



For the general features of the curve see curve A under the heading "For all the Curves."

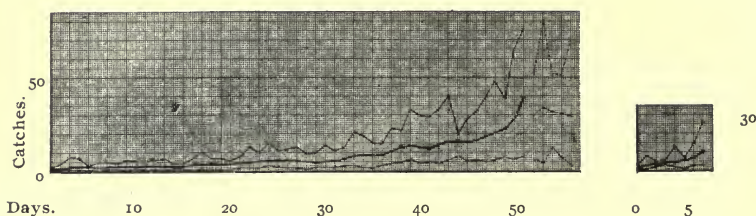
B played baseball when a boy and has since played tennis.

His steady rise without sudden jumps until the end is probably due to his effort to avoid consciously adopting a method of procedure. (See p. 215.)

His unusually high score on the next to the last day of his regular right hand practice was made during the April holidays, and on that day he was refreshed by a long walk into the country. The drop on the following (and last) day was accounted for by fatigue caused by working at his desk all the morning. (See p. 212.)

B did not take the monthly tests for determining the effect of intermission of practice.

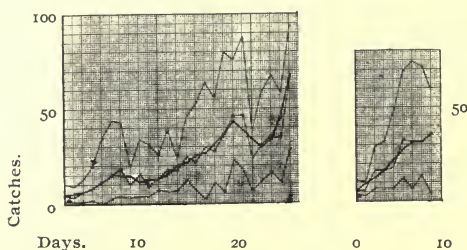
CURVE C.



For the general features of the curve see curve A under the heading "For all the Curves."

C never played baseball when a boy and he began his practice in ball-tossing by catching with his hands high in the air and the palms turned forward. His progress was very slow and on the thirty-third day he had evidently reached his limit by this method, the balls gliding down his hand before he could seize them. On the thirty-third day he consciously adopted a new method, holding his hand outstretched with the palm turned up. From this time he made a new start and his progress, though gradual, was steady. (See p. 214.)

CURVE D.

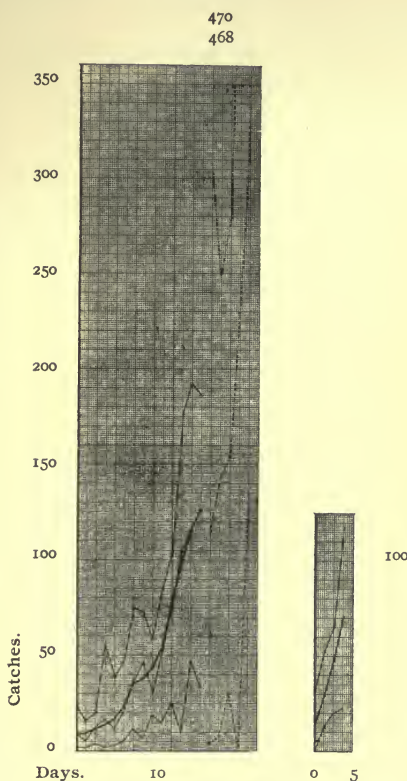


For the general features of the curve see Curve A under the heading "For all the Curves."

D was disturbed so frequently by sickness in his family that his curve is exceptional and shows the effect of loss of sleep on the learning process. He made regular progress during the first seven days and felt, as he said, that he had "the knack" when the effect of loss of sleep for several nights became apparent and he seemed to lose what he had gained. (See pp. 212 and 216-219.)

D did not take the monthly tests for determining the effect of intermission of practice.

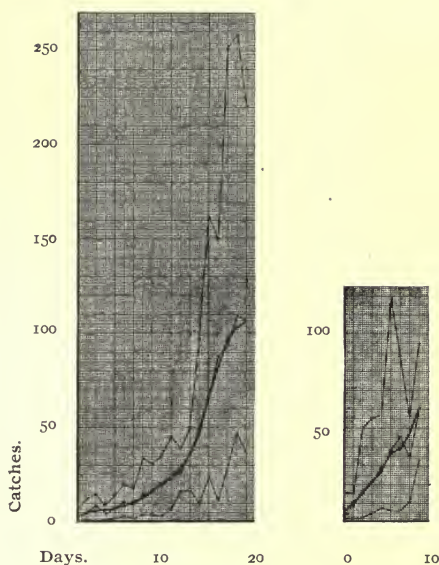
CURVE E.



For the general features of the curve, see Curve A, under the heading "For all the Curves."

E played baseball when a boy. In the tests he followed the plan of keeping the balls in two parallel columns in front of him. During the regular right-hand practice his scores were made with a great expenditure of energy, but after the regular practice ended and the monthly tests for determining the effect of intermission of training were made, his increasing skill became evident in the growing ease and grace with which he handled the balls. His highest throws on the last two monthly tests (the fourth and fifth month) were 468 and 470, while his average for these two trials was 229 and 337 respectively. Both of these averages, it will be observed, are far above his highest throw during the regular practice. During the last two "forgetting" months he took more or less exercise upon the tennis court. (See pp. 221-222.)

CURVE F.

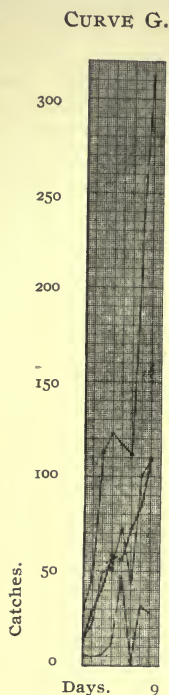


For the general feature of the curve see Curve A under the heading "For all the Curves."

On the third or fourth day of his regular right-hand practice F found himself tossing the balls up at nearly arms-length to the right and in such a way that they took a circular course and came down in front of him. The early adoption of a good method probably accounts in a large part for the uniform rise of his curve. (See p. 215.)

F played baseball when a boy but is not especially athletic.

F did not make the monthly tests of "forgetting."



G is the curve for learning to keep three balls in the air with two hands. The subject had practiced handling two balls with one hand on the tennis court, but had never tried three.

For the general features of the curve, see Curve A under the heading "For all the Curves."

On the sixth day G dropped from 730 catches in ten trials to 431. When he began the day's practice he thought that he was in good condition and confidently believed that he would make his first thousand, but he found himself unable to control his muscles. What had been easy the day before now required the greatest effort and at the close of the practice he was in an uncontrollable tremble. (See pp. 212-213.)

On the last day G made over a thousand catches in seven trials and then stopped. The cross on the ordinate for that day indicates what would have been his average at the same rate for ten trials.

G did not take the tests to determine the effect of intermission of practice.

None of the subjects gave any preliminary thought to the manner in which they might best get the knack of handling the balls, and this led to individual peculiarities of method, and ways of avoiding or meeting the common difficulties. One subject found himself very early in his practice avoiding collisions by tossing the balls up a little to his right and in such a way that they would take a circular course coming down in front of him. This method was most successful in avoiding collisions. Two others fell into the way of tossing the balls up at nearly arm's length in front so that they took a circular course toward the subject coming down closer to his body. The objection to this method was that the balls tended to fall too near the catcher and so constantly crowded him back. The other two kept the balls in parallel columns a foot or so apart and a little to the right of the median line of the body. With this method the balls got into a "mix up" periodically and then the subjects were obliged to toss high until they could straighten out the tangle when they would settle down again into the same parallel columns, but only to have the experience of trouble repeated in course of time. The same was also true in a measure of the circular throws. We shall return to these personal differences later in discussing the individual features of the learning curves. For other matters bearing on influences that affect the score see the section below headed "Some Conditions that Influence the Learning Process."

In the execution the eyes and attention were upon the balls in the air, indeed, upon them in the upper half of their course. Both the tossing and the catching were executed by the hand alone, for the most part practically outside the field of vision and of attention.

Results. While full numerical data are at hand, it has seemed to the writer that the essential features of the results could be made more easily intelligible by diagrams than by tables, and he has accordingly plotted the accompanying curves. All the curves are plotted in the same way and upon the same scale. Vertical distances show the number of balls caught; horizontal distances the successive days. The main curves show the progress of the right hand, while the dotted portion, immediately to the right of them, shows the result of the "forgetting tests." The lower curves at the extreme right present the progress of the left hand. Points on the uppermost (thin) line show the highest score reached in any single trial on the day in question. The lowest line of all shows, in the same way, the lowest single score from day to day. The thin line in the middle gives the daily average. The heavy line gives a smoothed average.¹

¹ The method used in smoothing was to average the averages for

Discussion of the Results. The curves just presented have certain characteristics in common.

1. With hardly an exception the curves are concave toward the vertical axis, which means, of course, that the progress was first slow and then more rapid. The curves which differ most from this type are those of B. In the case of his right hand curve the final rapid up-shoot is postponed and is not so conspicuous when it does come because of the steady, though gradual, ascent that has preceded it. His left hand curve was probably not carried far enough to reach the stage of rapid progress. It is altogether probable that all of the curves would in the end sweep more rapidly to the right and show a stage of slow progress as the physiological limit of skill in such matters was reached, but none of my subjects approached that limit. Bryan and Harter (6) found in their study of the acquisition of the telegraphic language a learning curve which had the rapid rise at the beginning followed by a period of retardation, and was thus convex to the vertical axis. The difference in form is very probably partly due to the difference in the type of learning involved, though it may also rest upon differences in the method of carrying out the test. This will be further discussed under the section headed "On Learning Short-hand."

2. All the curves show great irregularity of advance. Progress is never uniform but always by jumps. The learner seems to make no gain for several days or even longer, then he takes a leap perhaps to get a good grip and stay or may be to drop back a little. But if he loses his hold it is not for long and he soon makes this higher level his starting-point for new excursions.

A growing feeling of confidence usually preceded a permanent rise. The subjects felt that they were "going to do it."

There are not one or two special periods of delay in progress giving a "plateau" or two in the curve, as Bryan and Harter found in their study, in which successful co-ordinations are made automatic. Instead, there are many, the number varying with different individuals, and automatization is going on during the entire learning process. Miss Shinn found this same irregularity in the attempts of her little niece to learn to walk.

3. The average, holding at first somewhat closely to the lowest throw, is gradually drawn away from it by the growth in skill that reveals its reach in the highest throws. Though

the first three days, then those of the second, third and fourth days, and, again, those for the third, fourth and fifth, etc., to the end. By this method smoothed values cannot be found for the first and last days and in strictness they should have no place on the smoothed curve, though they are connected with it in these diagrams.

the lowest throw does not rise much above those of preceding days the number of low throws continually decreases. The lowest throws are more frequently the result of accidental conditions than the highest. While the latter is always above the learner's usual ability at a given time, it none the less shows the direction in which he is moving, and its height on any day bears some relation to his rate of progress. That is to say, though the learner will not reach the level of a given highest throw on the following day, he will shortly approximate it and get there permanently very soon. The curve of highest throws may in a sense be regarded as a curve of amateur proficiency while the curve of lowest throws may stand as the curve of professional mastery.

Some conditions that Influence the Learning Process in Ball Tossing. It has already been mentioned that execution is greatly affected by physical condition. It is well known that physical experts of all sorts must keep themselves in condition, else they will drop into the class of inferior men. The influence of this factor was evident in all of the subjects. The exceptionally high score of 2,155 catches in ten trials reached by B on the day before the last was made during the April holidays, and on the morning of that day he had taken a long walk into the country. On the following day when his score dropped to 1,359 he had been working at his desk all the morning and did not feel fresh. Everything required greater effort.

D lost sleep so often after the first four or five days, on account of sickness in his family, that his curve came to represent the effect of physical condition on progress rather than the learning process. When exhausted from loss of sleep he lost the skill that he had gained before.

Sometimes the lowered vitality is not apparent to the individual himself. G, who threw three balls with two hands, made a score of 730 catches on his fifth day and when he began on the sixth felt confident that he would make his first thousand. But, instead of this, he fell to 431. After starting he found that he could not control his muscles. What had been easy the day before was now done only with the greatest effort, and at the conclusion of the day's trial he was in an uncontrollable tremble.

Probably the "off days" that all subjects had belong here. These differed from the days when they were simply unable to reach a high score of the previous day. Sometimes they felt no confidence in their power to do any sort of good work though they could give no reason for the feeling. At times "warming up" freed them from this feeling, but again even

the lower scores required much greater effort, amounting in some cases to an exhausting strain.

The correctness of the curves obviously depends upon the uniformity of the effort put forth by the subjects. It is commonly assumed that the maximum effort is a constant factor for a given individual and that the only thing needed to secure it in a conscientious subject is to interest him in the work and then ask him to do his best. While I have no reason to doubt the conscientious effort of my subjects and the practical uniformity of the average effort of each from week to week, the matter is perhaps of sufficient general importance to the precision of psychological experiments to justify a little discussion of it. Even a direct interest in the results of an investigation aided by the no less effective acquired interest aroused by the professional advantage from a well worked out problem will not enable the experimenter himself to make a steady maximum effort in something that has little mental content.

In the ball-tossing the influence of this element was less noticeable on account of its use of the voluntary muscles and because of the counter-effect of the subjects watching their own progress and of competing with others. But even here unintentional relaxation became evident now and then by comparison with the intense effort put into the work at other times, as for example, in the last half when the score of the first was found lower than the subject had hoped for. Anderson (1) found, too, that in strength tests every man but two "failed to equal his best record when tested apart from the other members of the class," and Johnson (15) observed the same thing in tapping experiments. Yet it is easier to hold ourselves to steady intense effort in feats of muscular skill and strength than in many other things because of our mastery of the voluntary muscles. The effect on the short-hand practice (see the second study of this series) was unmistakable and there can be no doubt that it influenced the curve in spite of every effort to resist.

This lack of energy, due to waning interest, probably has more to do with delaying the learner's progress and making "plateaus" than anything else. One cannot escape a dead level in uninteresting work and after the enthusiasm that novelty stirs has spent itself the interest is dulled and effort slackens. This is a potent cause of the long dreary period of no progress in learning to speak a modern language or to play upon a musical instrument.

But the slow progress is frequently only an apparent one and due to our inability to measure the advance. It is a case in which figures tell only a part of the truth. In the ball-tossing during B's slowest period, when the curve showed little or

no rise, it was evident both to him and to the experimenter that he was still making progress and the proof of it, aside from unmeasurable observation, was his occasional high throws. The curve remained stationary because his imperfect training had not enabled him to meet the chance emergencies that were constantly arising.

But the matter of interest is still more complicated. In ball-tossing, after one has reached a fair degree of proficiency, the first part of each trial is always something of a bore till the fifty mark is passed when it becomes interesting. Later the interest may take another slump, rising again after the score has reached one hundred. At this point the possibility of an unusually high score keeps the subject alert to the end. There was also a plateau in the interest of some of the men after throwing for the first time a hundred in a single trial, for which they had been very keen. They felt that it was impossible to reach the two hundred mark at once, the total thousand was too far off to be alluring, and so the edge of their enthusiasm was turned. Later when the chance to make another record seemed good they became as alert as ever. Indeed, after the satisfaction of having thrown a hundred had subsided, and the work continued for a time without great progress, the first twenty-five took on an acquired interest in the anxiety to get done. This brought greater care.

The whole question of maximum effort is well worth special investigation.

The confidence that follows a successful series of throws proved of considerable value, unless it led to the carelessness of over-confidence. Faith in one's ability to get out of a desperate situation in the tossing increases with success. This leaves the attention imperturbed, and one does not "go to pieces."

A long period of delay often represents the physiological limit with a particular method of tossing and a rise is made only by the introduction of an improved mode of procedure. This was especially noticeable in C who caught at first with the hand high in the air and the palm forward and almost perpendicular. This high catching seemed to be a sub-conscious accommodation to the position for throwing. The balls, of course, glided down his hand before he could seize them and he made practically no advance until he held his hand lower with the palm turned upward. This improvement, which necessitated the further change of keeping the balls at a distance from his body, was consciously adopted on the thirty-third day and at that time a new rise began. The general flatness of C's curve is doubtless due to the fact that he never played ball when a boy.

F, on the other hand, on the third or fourth day found himself tossing the balls up at nearly arm's length to the right and in such a way that they took a circular course and came down in front of his body. In this way collisions were avoided. The plan entered upon unconsciously was then consciously adopted, and as a result of finding a successful plan early in the work his progress was rapid and his curve is the most regular of the lot. It may probably be regarded as typical for muscular feats in which there is no long continued feeling around for a successful way of doing the thing, as when the learner is assisted by a good teacher.

B, again, tried pretty constantly, throughout, not to consciously adopt any method, but to let everything take its natural course and this seems to be the reason for his slow but steady ascent without any high jump until near the end. His efforts in this direction did not, however, prevent his final approximation to a regular method though one less advantageous than that developed by F.

We see in this the value of suggesting good ways of doing things while the learning is still in its early stages. If the learner goes on he will finally develop a plan of his own but only after a good deal of wandering and even then it may not be the best. But the suggestion to be effective should be given at the time when need for it is keenest, at the "psychological moment." It is then that its value will be felt.¹

In polo, golf, baseball or football good form is absolutely necessary for reaching a high degree of skill. It is the essential prerequisite for good methods. Movement and position become associated and a change in the latter requires relearning the former. The physiological limit of bad form is low.

In learning any complicated performance, we progress by sections. That is to say, the co-operating movements improve separately. This leaves certain errors conspicuous when we are well along in the work. Indeed the whole learning process, at least in learning to toss and catch balls, seems to consist in eliminating errors. First the obvious ones are gotten rid of, then new ones appear, and it is only after all have been overcome that the thing can be regarded as mastered.

In avoiding errors there was adaptation, apparently more organic than conscious, to conditions and often it was so delicate as to elude observation. B, for example, found himself tossing high in order to have time to recover from a difficult situation, and at another time he caught himself putting his body into a more alert position by slightly raising himself on

¹ Miss Shinn reports that her niece until six years old always ran flat-footed, but when she was shown the advantage of keeping her heel slightly lifted, she readily adopted it.

his toes and making his muscles tense. Then he realized that he had been doing it for several days. So far as he could determine consciousness had no originating part in it.

It is interesting that all the subjects improved by hitting upon better ways of working without any further conscious selection, at first, than the general effort to succeed. There seems to be a competition of methods. Just how this selection occurs without conscious interference is not easy to say. Consciousness discovers modes of action already in use and selects some of them for survival because of their success. They then pass into the automatic. In this way reflex movements may have first been conscious.

Two learned to throw in less than half the time that the others needed, but their movements always called for a great outlay of energy. Economy of effort is an important element in effectiveness, but its getting requires time for the solidification of associations and for the elimination of useless movements, with the subsequent automatization of just those that are essential to the process.

A certain amount of "warming up" was usually necessary. While high throws were not confined to one part of the day's trial, they rarely came at the beginning. Commonly, so long as the score was low enough to eliminate the effect of fatigue, the one or two high throws after the warming up period were followed by a slump which again yielded to high ones toward the end. This form of the daily curve was too common to be entirely a matter of chance. It is another case of the uncontrollable variation of the maximum effort.

In his experiments on Practice and Habit, Johnson (14), also, noticed that his subjects could not get control of their muscles within the time of the preliminary tests.

Bryan and Harter have told us that it is intense effort that counts, and this is true, but with a qualification. Throughout this investigation it was clear that attempts to spurt were not effective. Indeed, the very effort interfered with success by making the attention too obtrusive. Special strain is itself a distraction, as Johnson found it at times. It is steady and calm intensity that counts for progress.

The importance of strenuous effort lies in the fact that up to a certain point of intensity it is generally *successful* effort and that is what counts, as Woodworth (25) and Johnson (14) found.

Fatigue from any cause not only brings a lowering of the day's score but the entire process of learning is probably hindered. The growth of the nervous system into the required forms of activity has been disturbed.

D felt that he had caught the knack when sickness in his

family brought loss of sleep, and it was nearly a week before the feeling of confidence returned. Meanwhile his score dropped, except for occasional spurts, far below that which we should expect from his previous and subsequent record or from that of others.

F, too, felt that he was delaying his left hand progress by practicing when fatigued from lectures, and a change of hour brought immediate results.

In tossing and catching the ball a pretty general co-operation of all the muscles of the body is required, though those, of course, that effect the movements of the arm and hand are most directly involved. Of these the movements most prominent in consciousness are the general movements of the arm. The body movements, in most cases, do not come into consciousness at all, and the finer movements of the fingers and hands, except so far as they are covered by the general intention to toss and catch the ball successfully, are almost equally unregarded. As a matter of fact, however, skill in throwing and catching is rather more an affair of the fingers than of any other members.

The question then arises how are the necessary co-ordinations brought about? It does not seem difficult to bring the matter into line with phenomena already pretty well known. Let us suppose a successful toss and catch is made. This is followed by a double effect; it leaves, as every action does, a trace in the nervous system which facilitates later repetition of the same action, and the successful adaptation also gives rise to a feeling of pleasure. The effect of pleasurable sensation is a heightening of muscular tonicity or a general tendency to motor discharge, which in the case of an action just performed—one whose neural effects are still lingering—is equivalent to a partial reinnervation of the same co-ordinated group of muscles which again deepens the existing trace. The next actual effort finds the nervous mechanism a little readier to react in this favorable way. In case of an effort that does not lead to success, the slight displeasure at failure exerts its natural depressant effect upon the whole neuro-muscular system, and this does not deepen the neural trace left by the original movement, and even, perhaps, breaks up the incipient co-ordinations that gave it its particular form. In any case, whatever its mode of action, it has not the reinvigorating effect upon the original neural trace exercised by the pleasurable sensation, and therefore, in the long run, the successful movements, and the co-ordinations upon which they depend, tend to persist while those that are unsuccessful tend to fall away.

Now it will be observed that this action of pleasant and unpleasant sensation does not depend at all on consciousness of the detail of the movements and applies as well to a movement

of which all the ultimate factors are unconscious and only the general end known. In such a movement, if the result is unsuccessful, a slightly different movement follows at the next trial, that is, one in which the co-ordination among the muscles engaged is slightly altered, as an automatic result of the partial inhibition of ill success. This new trial may be no better than the previous one, in which case it is again altered until success is reached or the attempt given up completely. In the case of movements the details of which come more or less completely into consciousness the same process goes on but with more rapid progress toward the desired end, because the variations from which the advantageous movements are selected are not chance variations, but are from the start more or less perfectly suited to the requirements of the case. In the ball tossing the general arm movements remain the prominent thing in consciousness, as we have said, while the finger movements are little noticed or quite neglected, and yet, nevertheless, the whole co-ordinated group is worked over, under the influence of the voluntary movements, into proper adaptation for the successful performance of the feat.

2. *The Effect of Right Hand Training upon the Skill of the Left Hand.* As already described (section above) the subjects in this investigation were all tested with their left hand before the right-hand practice began, in order that the effect of the latter upon the former for the left-hand tests, which stand at the right side of the charts given above, might be determined. In the curves the short straight line projecting to the right of the vertical axis shows the score made by the left hand in this preliminary test. The progress of the left hand in its subsequent practice is shown upon the same scale and in the same way as that of the right hand by the light and heavy lines of the diagrams.

Several things are at once noticeable.

1. The record of the first day of regular left hand training is in all cases higher than the preliminary test, though in no case had the left hand been practiced with the balls at all during the interval. More than this, the score never drops to the level of this preliminary test, which shows that the gain was permanent.

2. The left hand curves bear a striking resemblance in general form to the corresponding right hand ones with this difference that in all but one case they ascend much more rapidly. A did in eight days with his left hand what his right hand needed thirty-eight days to accomplish. E made a left hand record in four days that he had not been able to do in less than eleven days with his right. The difference with the others is not so marked, but in all cases but one the left hand curve rises more rapidly than the right.

3. All of the subjects but one made a better score with their left hand on the first day of its regular practice than they had been able to do with their right at the beginning of the work.

4. The highest and lowest left hand single throws are in almost all instances higher than corresponding right hand throws on corresponding days.

F was delayed in his left hand progress at about the middle of the work by physical and mental exhaustion. The anomalous record of B will be considered a little later in another connection.

The conclusion is unavoidable that in the majority of cases the training of the right hand was somehow effective upon the left also. The same general result has been noticed by many observers engaged in different lines of investigation, and has been made the subject of a special investigation by Davis.¹ The chief point of interest is to discover how the effect is produced. Is it due to some purely peripheral change, or to some alteration in the central nervous system, or finally to some method or plan of work that can be applied equally well in the case of either hand, as for example the knowledge of spelling which a man could use as well in writing in mirror script as in the ordinary way? It is not impossible that cases could be found that would exhibit the co-operation of all three. In the ball tossing there was evidence, certainly, of the last two. All the subjects were able to make use with the left hand of the methods of handling the balls, and of recovering control of them after an ill-directed throw, that had been developed in the right hand practice. In all the cases but one a good deal of a less conscious facility (of a sort that might indicate some kind of symmetrical training of the central nervous system) was probably present. In the case of B, whose record supplies the anomalous case in the left hand training, the "method" of tossing and managing the balls was distinctly carried over, but the less conscious neural habits were apparently not present. He could get out of difficulties with remarkable dexterity, but he failed in the simple things. He could not use these "higher habits" to the best advantage because he did not have the lower, and in his case these came with more than usual difficulty. The others were able at once to build in the sub-structure of central (or neuro-muscular) skill and so to learn the art of left hand throwing much quicker than the right. The mental element, the power to comprehend and meet a situation, is evidently, then, in most cases, the more difficult part of complex muscular feats of skill, since the right hand, if taken first, needs so much more time for the learning than the left,

¹See 9 (bibliography) also 10, 11, 18, 20, 21, 22, 23 and 24.

notwithstanding its greater general facility in such movements in right handed people.

The anomalous nature of B's left hand curve led to the inquiry as to whether he was relatively less skillful with his left hand than the other subjects. For testing this a target approximately seven feet six inches in diameter, with nine concentric circles each about four and a half inches wide, was used. The target had previously been used by A. W. Tret-tien. The test consisted in throwing one hundred balls with each hand from a distance of thirty feet. The bulls-eye was nine inches in diameter and the balls used were such as have been described above in the section on ball-tossing. The following shows the success of the left hand compared with the right in percentages. C = 45 per cent., B = 50 per cent., F = 66 per cent. while A and E each gave 72 per cent. It is evident from this that, with the exception of C, B was relatively less skillful with his left hand than the others. C did not continue long enough with his left hand in the regular work to enable us to say just how his curve would finally have compared with that of B. If now we consider the skill of the right hand, merely, estimating it by success in throwing at the target, we have the following order, beginning with the best. F, E, B, C and A. Again, arranging the subjects in the order of their left hand skill, leaving the right hand out of consideration, we have, beginning as before with the best, E, F, A, B and C. It is evident from this that left hand deficiency is, at any rate, one cause of the anomalous form of B's left hand curve.

To return, now, to the subject of left hand training, it would be a mistake to suppose that such experiments in cross-education give support to the doctrine of "formal education." There is no evidence to show that training has general value. Indeed it all argues strongly for the influence of content. Volkmann (22) found that six months of regular practice in distinguishing small visual distances in which his eyes gained remarkable power, had no effect whatever on his ability to perceive small tactual differences. The right hand has had a great variety of training that ought to bring it along rapidly in ball-tossing on the principle of formal training, but this investigation shows just the reverse. The right hand learns it very slowly but the special training that comes from doing it enables the left hand, awkward and stiff as it is, to get control of the situation in about one-third of the time required by the right. Skill in certain lines may be serviceable in other similar processes, but its value decreases as the difference between the kinds of work increases, and in many cases it is probably reduced to zero.¹

¹ For a full discussion of this matter, with experiments, see the work of Thorndike and Woodworth (26) and Bair (3).

3. *The Effect on Skill of the Intermission of Training.* To find out how rapidly a feat of muscular skill could be forgotten three subjects were tested with their right hand one month after they had finished with their left and the test was repeated every thirty days for five months. The curve is given in the chart by the dotted lines between those for the right and left hand.

Instead of being a forgetting curve, it turned out to be a new curve of learning though the subjects did not touch the balls during the intervals.

During the second and third month E rose steadily in skill, his average and highest throws greatly exceeding those made at any time during the regular practice, while in the fourth month his average exceeded his highest previous throw and in the fifth month he made the astonishing average record of 337 catches in ten trials. In this test his lowest score was 135 catches, and, with the exception of this and one of 196, none of his scores fell below 300 while three exceeded 400 catches. A began his rise the third month, and with him, too, the highest throws were far above those made during the regular work. C's score just about held its own with that of the regular practice. Evidently the mind not only grows to the modes in which it is exercised but this mental growth may continue, for a time at least, after the practice has ceased.

Prolonged practice in any muscular exercise brings increase in the size (or efficiency) of the muscles as well as increase of skill in their use. This portion of the ball-tossing study gives evidence of the relative independence of the two. The point at which fatigue came during the regular practice, varied somewhat, of course, with different subjects, but it was first felt after about forty or fifty throws. If the subject recovered from this, he felt comfortable until he approached ninety or one hundred, when fatigue came for the second time. If he pulled through this, he was all right for another fifty, at least so far as fatigue was concerned, and so periods of vigor and fatigue alternated about every fifty throws. But during the experiments in "forgetting" fatigue came on earlier and at times was so exhausting that the subject could not continue. But skill during this time, in which muscular endurance decreased so rapidly, suffered no decline. Indeed, in two of the three subjects it greatly increased during the thirty day omission of practice.

That fatigue prevented us from seeing the full increase of skill during these months in which practice was omitted is evident from the fact that E made his remarkable series of the fourth month when fatigue was much less noticeable than in the three preceding tests, because he had been playing tennis

daily during the previous week. Then, again, in the interval between the tests of the fourth and fifth months he played tennis quite regularly and his unprecedented score at the last of these trials, five months after the end of the regular practice, shows a further increase in skill during the intermission of training, notwithstanding his loss in muscular endurance previous to exercising on the tennis court.

That there should have been little loss is not very strange, for it is well known that feats of bodily skill, like swimming, dancing or bicycling, are not wholly lost by long periods of disuse, but we were not prepared for a positive gain in skill. It is interesting in this connection to cite Houdin's (13) experience who tells us that having in the past learned to handle four balls while reading he was still able to keep three in the air and read at the same time though he had "scarcely once touched the balls during the thirty years preceding." And in *L'Année psychologique* (5) Bourdon reports experiments showing that there was not only no loss in skill in mental processes after an interruption of the training for a period varying from twenty-eight to thirty-eight days and longer, but in most instances there was a positive gain during the intermission, while facility in certain mental processes was not wholly lost after seven or eight years' omission of practice.

SUMMARY OF RESULTS AND GENERAL INFERENCES.

1. The curve for learning a feat of muscular skill, so far as this study may be regarded as typical, is concave toward the vertical axis. (See p. 211.)
2. Progress is never steady but always by jumps, with not one or two but many intervening periods of delay. (See p. 211.)
3. Practice with one hand trains the other, as has already been observed by earlier investigators. (See pp. 218-220.)
4. The gain seems to be due in part to the possibility of the transference of many points of "method" and their application to the throwing of either hand and in part, also, to a more direct effect of training, probably upon symmetrical portions of the nervous system. (See pp. 219-220.)
5. Method was hit upon and improved by the subjects of these tests at first without conscious intent.
6. The general physical condition of the subject greatly influences his skill in ball-tossing as well as the effectiveness of his practice from the point of view of acquisition. (See curves G and D and p. 212.)
7. Maximum effort, in spite of conscientious intentions to the contrary, is a variable standard, and emotional factors of various sorts probably affect the score and by inference the learning process as well. (See pp. 213-214.)

PEDAGOGICAL HINTS AND SUGGESTIONS.

It would hardly be possible for one interested in education to carry through a study of this sort and not bring away a plentiful crop of pedagogical hints and suggestions, some, perhaps, mere analogues of little worth, but others that point the way to interesting lines of investigation. Without attempting to defend or enlarge upon any of them the writer may set down the following.

1. Growth in muscular skill stimulates intellectual development in the final determination of the method. We get into ways of doing things unconsciously, find ourselves doing them, in fact, but later those that survive do so for a reason. This is important in the development of children through play.

2. The effect on the experimenter of watching the lowest and highest throws is interesting. Teachers are apt to estimate a good pupil by his best achievements, forgetting his comparatively few failures, while a poor pupil is estimated by his poorest work. The best work is impressed upon our mind in the one case and the poorest in the other. It has the same psychological basis as the saying "it always rains when I do not take an umbrella."

3. Monotony.

It is a platitude, which nevertheless must continually be dinned into the deaf ears of schoolmasters, that children do not see things as adults do, and to them future benefit is at best a poor recompense for present misery. If remote interests often count for little with adults they surely have even less force with children.

4. The effect of fatigue during the learning process has not received the attention that it deserves. Most of our tests of fatigue on school children, so far as they relate to work, have been made in connection with what the child had learned long before and which had become automatic.

If, as Woodworth (25) found and as these experiments seem to show, it is not mere practice but successful practice that counts, then school work continued to the point of fatigue is disastrous.

5. The influence on their general progress of watching their own advance from day to day was undeniable in the case of my subjects and it suggests the question of introducing it into school work. As it is now children are always tested by new demands or by comparison with companions who are going along with them. On this account they often feel that they are making no advance, because their marks do not show any, but if they could see their own curve grow from day to day they would not only be interested in its variations but would also be convinced of their progress.

6. Children evidently cannot be expected to make even slow continuous progress. They must have time to catch up with themselves, so to speak. There is evidence all through this investigation that there are moments when we run ahead of our power and delays are necessary that associations and habits may have time to set.

7. Bourdon's experiments, as well as this investigation, raise the question of how many recitations per week in a given subject will be most advantageous for children. It is by no means certain that one each school day brings the best results. They should, of course, be frequent enough to prevent loss of interest but also far enough apart to give sub-conscious processes a chance. Special investigations are needed here.

8. Periods of retardation may represent the physiological or psychological limit for the method used.

9. The fact that attempts to spurt instead of making for progress delayed it by bringing into prominence psychical activities that serve the learner best while in the background, counts strongly against cramming.

10. Suggestion of a good way to do the thing saves time that would otherwise be lost, but the suggestions must be made when the learner feels the need for them.

II. ON LEARNING SHORT-HAND.

When short-hand was selected to illustrate the more strictly mental side of learning, it was thought that this might give a typical curve and that, in any case, it would furnish an interesting parallel to the research of Bryan and Harter on the learning of telegraphy already referred to. The practice and tests, however, seemed to show that each branch of knowledge has its own characteristics which so greatly affect the learning process that we can speak of a typical curve of learning only in a very general way.

For this test but a single subject (the writer) was available.

An hour and a half each day was given to the study and practice of short-hand and the study was continued through something over ten weeks. During the first half the study time was spent in writing, but in the latter half it was about equally divided every day between practice in writing and in reading what had just been written.

The subject was tested daily, at first in writing only; later in both reading and writing. In the writing tests James' Talks to Teachers was used for dictation. There is no doubt that his rich vocabulary had an effect upon the curve. Had some author with a less extensive range been taken it is probable that a more rapid rise would have commenced at about the second third and the ascent then have proceeded with continu-

ally increasing rapidity till the limit was approached, when it would have passed over into the usual physiological plateau.

An assistant read a few words, usually a phrase, then waited until, by pronouncing the last word, the subject indicated that he was ready for more.

The subject wrote as fast as he could for a period of ten minutes and his score for the day was then measured by counting the words and also the lines and parts of a line in order to equalize, so far as possible, the effect of long and short words.

The tests in writing began February 18th and were taken each day until April 4th after which they were omitted on Sundays. They closed April 30th. The reading tests did not begin until March 8th and there were two omissions, March 10th and 27th. In other respects they corresponded with the writing tests.

The method at first adopted in the reading tests was for the subject to write down the words in long hand as he read them. The length of the test period was, again, ten minutes but only ten seconds were allowed for a word. If the subject did not indicate within that time that he had the word time was called and he was required to pass on to the next.¹ At the end of the ten minutes his written work was compared with the text and the number of words and lines correctly read was recorded. When about two-thirds through, however, the limit by this method was reached on account of the time taken in writing. This point is indicated by the break in the curve. On the eleven following days we alternated between the plan of having the subject write the words as before and that of having him read them aloud while the assistant followed in the text noting the mistakes. After that the latter method was followed to the end. The curve for translating without writing is shown in dotted lines in the diagram below.

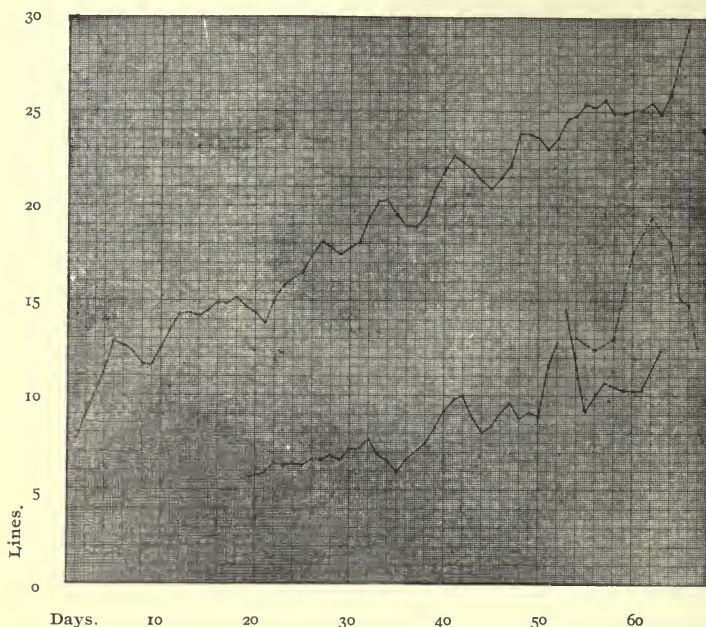
The material for the reading test was that which the subject had himself written ten days or more before, and so it had the natural difficulties increased by poor and sometimes incorrect writing. It was fully "cold" and only once or twice was the reader helped by memory and then only in one or two sentences by the general idea. The Pernin system of short-hand was used and no attempt was made to abbreviate by phrasing, as that would have introduced a new element.

The upper (writing) curve is smoothed by threes as before (see foot note p. 210) but that for reading is smoothed only to

¹The time was not always called with perfect regularity. An ordinary watch was used and the assistant found it difficult to carry the task through the full ten minute period without slips, but the average time was pretty fairly kept and the differences due to this cause are probably insignificant.

the break. From that point the curve is drawn from the records for each day.

CURVES FOR LEARNING SHORT-HAND.



The curves for learning to write short-hand is above and that for reading the symbols below. The number and parts of lines written or read are shown on the vertical axis, the days on the horizontal. The break in the reading curve on the thirty-fourth day indicates the time when the subject began to alternate between writing the words in long hand as he read them, and reading them aloud while an assistant followed in the text noting the errors. From this point the curve gotten by the former method is given in the unbroken line as before, while the dotted line shows the progress when the score was kept by the latter method. (See p. 225.)

In the curve for writing (the upper one), the first rise from complete inability is rapid because the acquisition of a few imperfect co-ordinations and associations is easy and helps enormously when one starts at the zero point. Very soon, however, the subject makes all the gain possible of this rapid sort and then the rise settles down to a more or less gradual ascent. This curve would naturally outstrip that for reading at the start because the writer knows what he is to say and is not delayed by wrong translation of a preceding word, as in reading. Relieved of these embarrassments the writer in short-hand

and, probably, the sender in telegraphy, can give his whole attention to getting off the word before him.

After the force of this initial rise is spent, retardation alternates with progress. The difference between this beginning rise and the periods of lesser gain that follow is that a little gain makes more showing at the start. There is no evidence furnished by this investigation of a 'special time for the formation of "higher habits," or association groups. When there is continued arrests in progress its sufficient cause, in the writer's opinion, is to be found in the emotional factors of learning. This will be further discussed below.

In ball-tossing the initial rise was practically absent, and in learning short-hand it lasted for only a few days.

Bryan and Harter (7), in the receiving curve for telegraphy, found a rapid initial rise followed by a period of little or no progress, or, as they term it a "plateau," and they conclude that "learning to receive the telegraphic language consists in acquiring a hierarchy of psychophysical habits" and that "a plateau in the curve means that the lower-order habits are approaching their maximum development, but are not yet sufficiently automatic to leave the attention free to attack the higher-order habits."

The nearest parallel in my work to telegraphic receiving is the reading of the short-hand notes. A glance at the chart will show that nothing of the initial rise is to be found in the curve for reading and that there is nothing also that looks like a "plateau." The short-hand writing is more nearly comparable with the telegraphic sending, the curve for which as given by Bryan and Harter¹ shows a rapid rise at the beginning, but no such rise is apparent in my writing curve except for the first few days while the symbols were being learned. (4.)

Of course the amount of daily practice in ball-tossing, as managed in my experiments, increased as skill was gained. The learner threw more balls before he missed. But this increase of practice with growing skill is characteristic of all learning. A beginner in short-hand, and in telegraphy also, gets in more practice to the hour the second week than the first. So the ball-tossing was not wholly exceptional in this respect. In order, however, to see the effect of equal amounts of practice, A's curve was replotted in such a way as to show the progress per thousand throws. Even this failed to show convexity.

¹ None of the curves for learning to toss balls, as already noticed, have the initial rise with the succeeding "plateau" that characterize Bryan and Harter's curves.

The writer does not wish to dispute the gradual formation of such a hierarchy of habits as Bryan and Harter have described, nor to call the accuracy of their work in question, but he believes that the explanation of the order and the method of formation of such habits is different from that suggested by these authors and in particular that the "plateaus," when they occur, are to be accounted for in other ways. This immediate rapid rise at the beginning seems to be true only of these things that have symbols or other devices for handling and presenting ideas, and it is probable that after this first spurt, the length of which would vary with different sorts of material, the general form of the curve for learning is concave until the physiological limit is approached. Telegraphing involves fewer symbols, and the distraction of deciding on sounds and abbreviations, that mark the learning of the Pernin short-hand system, would not so greatly disturb the beginner, and so, having less thinking and deciding to do at the start, the learner in telegraphy would probably go on improving without great set backs longer than the short-hand writer.

In reading short-hand it was found that the speed rose with great rapidity whenever the reader was able to get the context. So long as he read by words, every word had to stand for itself, but when he got control of enough words to give him the sense it was no longer necessary to recognize each word. He could even correct in the reading those that were wrongly written.

The effect of getting the context was seen in occasional spurts soon after the reading tests began. Later they became more frequent.

The material for testing was taken as it chanced to come in the book, and the sudden drop in the reading curve at the end was due to its unusual difficulty.

In learning short-hand, and presumably also in learning to receive or send telegraphically, a large number of associations are formed that do not affect the speed of work, because there is no opportunity to use them and the learner seems to make little or no progress, not because this is the particular time for the formation of a "hierarchy of habits," for this is going on all the time, but because the range of association knowledge in the subject is too limited to meet the demand. After enough has been accumulated to meet the demands of the tests the curve will rise more rapidly. The associations that have been forming from the start have now become numerous enough to be effective.

The essential thing in getting a purchase here is to learn enough to make the associations already formed available. When this is done the rise comes.

That the failure of this material to influence the curve earlier is not altogether due to the lack of higher association-habits is evident from the fact that whenever the context is gotten, symbols that had long lain unused and were almost forgotten come in for service with the rest.

Associations are more active during the periods of delay only in the sense that there is more material to work upon than before, since they are not present until after the first rise that accompanies the rapid gathering of symbols at the start, but this is true of any later stage in the process when compared with an earlier one.

Bryan and Harter (6) tell us that in telegraphing "the learner enjoys the practice of sending, but feels practice in receiving to be painful and fatiguing drudgery." Unfortunately this feeling cannot be gotten rid of by setting aside certain periods for work. Practice is less effective and ennui may reach such a degree that little or nothing is accomplished during the hours of work. The effect of this monotony could be seen in the ball-tossing, but it was less influential there for reasons already given.¹ In the hours of practice upon the shorthand, however, its influence was great and wholly uncontrollable. The lack of content made the material so dry that the attention would wander in spite of every effort on the part of the subject to make his periods of practice equally valuable.

I can hardly doubt that this emotional factor was largely influential in making the plateau that Bryan and Harter found. Visible success is always interesting to the one who succeeds, but after the first dash has been made, and the easy acquisitions have been gathered in, the rest of the work involves a good deal of drudgery and all the indirect interests that may be brought into play cannot wholly relieve it of this drag. The beginner in German is fully conscious of the initial rise and thinks it great fun to say "*Guten morgen*" and "*Wie befinden Sie sich heute*" and his progress has certainly been great and rapid when compared with the zero with which he started, but the fun ends and the depressing monotony weigh him down long before he has reached the point where he can understand a native.

The wearying monotony and discouragement of this intervening period lessens the efficiency of the work which was so effective during the first spurt. Then, when the learner has reached the point where he can get the context and guess successfully, his practice increases without conscious effort and the pleasure of success causes him to redouble his efforts.

Psychical adaptation, too, greatly affects the curve and it is

¹ See p. 213 above.

probably this almost unconquerable tendency to remain in the stage that just meets our needs, rather than the lack of higher association-habits, that makes it so difficult for an operator to rise above the skill necessary for his own office work and that prevents years of daily practice from bringing a man to his own maximum ability to receive. Johnson (14) also thinks that Bryan and Harter's plateaus are "resting places in the effort," and in a large measure this is probably true.

Summary. Some points of agreement in motor and mental learning may be set down here by way of summary.

1. As in ball-tossing the learner seems to make no advance for a time and then springs to a higher level, perhaps only to fall back a little but, at all events, not to go higher until he has strengthened his position here.

2. I have found no evidence for one or two special periods of delay in progress in which preparation is made for a higher order of habits. As before, automatization is going on throughout the process.

3. Here, too, consciousness discovers certain methods in operation and approves or disapproves of them. So improvement goes on through elimination and selection.

4. The effect of variations of maximum effort was apparent also in the tests with short-hand, and here the periods of discouragement did not always coincide with the 'plateaus.' Several times the subject was confident that he had beaten his record until the count proved that he was still on his old level.

5. Physical condition was a no less determining force than before. Many times the drop could be clearly traced to it.

6. As with the balls over strained attention was a hindrance. Signs that were gotten incidentally and used occasionally, without special effort to fix them, were remembered better than those upon which a good deal of effort was expended. Effort defeated its own end by calling into consciousness other similar signs.

Symbols just learned, and which could be used with considerable facility when the subject was alone, could not be recalled under the tension of the test. The very fact of being tested had an inhibiting influency on the mental processes.

The pedagogical bearing of these observations is obvious.

III. ON THE ORIGIN AND CONTROL OF THE REFLEX WINK.

The experiments on control were the first undertaken and it will be convenient to consider them here.

Darwin tells somewhere of standing before a cage of snakes with his face close to the glass and finding himself unable to resist the instinctive tendency to protect his eyes from harm by winking and jumping back when they struck against the glass,

and that, too, though fully conscious that the plate glass was thick enough to absolutely remove all danger.

Since the wink is a protective reflex of the greatest importance for the protection of sight in animals with movable eyelids, and so for their preservation, it is of no little interest to determine under what conditions it is amenable to the will. Some of these conditions had already been investigated by Partridge (17).

In the following experiments which were directed to a more detailed study of the means by which control of the wink is learned the subjects were, again, university students and one professor.

The apparatus consisted of a framed piece of plate-glass about six by eight inches in size which was attached to a steel rod supported on four legs. A small wooden-headed hammer was attached to the lower part of the frame behind the glass in such a way that when it was pulled down and released by the experimenter it would fly up and strike the plate glass. The wooden head of the hammer was covered with cloth and a strip of rubber so as to reduce the sound somewhat and lessen the danger of cracking the glass.¹

In the first series of experiments the stimulus was both visual and auditory, no further effort being made to lessen the noise caused by the blow of the hammer or the rattle of the apparatus than that described above. The subject sat with his face close to the glass and his chin resting on a frame support. Effort was made to keep from winking but there was no strain of the attention or muscles. In order to determine whether the period of the day had any noticeable effect, tests were made in the morning before the subject began his work and again at noon and, finally, they were repeated late in the afternoon just at the close of the day's work. They were continued under these conditions for five days.

Results. The time of day and such fatigue as comes from an ordinary day's mental work had no appreciable effect. Three subjects tested this by reacting in the morning, before the day's work, at noon and finally late in the afternoon.

First day { 1st Series of 20 trials—W = 9, P = 0, I = 0.
2nd " " " —W = 14, P = 0, I = 0.

N. B. Throughout these experiments W = full wink, P = partial wink, *i. e.*, the least noticeable eye movement in response to the excitation and I = inhibition of the wink. After a little practice the experimenter was able to distinguish four degrees of winking, depending upon the force of the movement and completeness of the process. It

¹The apparatus was at first the same as used by Partridge, but as the experiments progressed it was altered from time to time in minor details.

has seemed best, however, to consider only the full wink and the least noticeable reaction to the stimulus.

Fifth day { 1st Series of 20 trials—W = 2, P = 7, I = 3.
 2nd " " " —W = 1, P = 12, I = 3.

A series of artificial devices for distracting the attention was then tried with the same subject. Some of these diversions were winking fast between the strokes of the hammer, biting the lips and refraining from swallowing the saliva until it became annoying. These distractions were very effective when first adopted but soon lost their power. The following are samples of reactions made on different days under the influence of varying distractions. The entire series occupied seven days. Distractions of this sort were rarely effective longer than two days.

20 trials—W = 0, P = 17, I = 2.

20 trials—W = 0, P = 5, I = 12.

20 trials—W = 0, P = 3, I = 17.

20 trials—W = 0, P = 3, I = 17.

20 trials—W = 1, P = 7, I = 12.

At this point a new subject was taken and he was instructed to put his attention on inhibiting the wink but to avoid any muscular strain. As there are great individual differences it does not seem best to compare the reactions of different subjects but rather to follow the attempts at inhibition of each subject under varying conditions.

As before, the following are the reactions for the first day. The tests were made in the morning, at noon, and late in the afternoon.

1. 20 trials—W = 7, P = 3, I = 0.

2. 20 trials—W = 1, P = 15, I = 1.

3. 20 trials—W = 5, P = 11, I = 0.

4. 20 trials—W = 0, P = 10, I = 4.

5. 20 trials—W = 7, P = 4, I = 3.

6. 20 trials—W = 1, P = 10, I = 8.

Six days' practice brought no improvement. The result on the last day was

1. 20 trials—W = 4, P = 2, I = 2.

2. 20 trials—W = 0, P = 9, I = 5.

The same subject now tried the effect of focusing his attention on a point on the wall behind the hammer and directly in its path. In another series he thought of some just discernible dots on the wall at the same time keeping his eyes fixed upon them. The purpose was to withdraw the attention from the excitation. The improvement over the reactions when the attention was on inhibiting was marked. These experiments, which occupied four days and included two hundred and

eighty excitations in series of twenty each, caused a total of only fourteen full winks. The following are samples taken from the first and last days.

1. 20 trials—W = 5, P = 12, I = 2.
2. 20 trials—W = 0, P = 7, I = 13.
3. 20 trials—W = 0, P = 13, I = 5.
4. 20 trials—W = 3, P = 6, I = 5.
5. 20 trials—W = 1, P = 19, I = 0.

It may be mentioned here that very little improvement was observed so long as effort was directed merely to inhibition. Control seemed to come in proportion as excitation and inhibition were forgotten.

A series lasting four days, in which the attention was given to the thought of keeping his attention on a black target behind the hammer, gave practically the same results as the series that we have just considered.

The effect of reducing the sound by closing his ears with his index fingers was now tried. Series with the ears open were alternated, but with some irregularity, with those in which they were closed. Six days were given to these and the following show the results of the first and last. The attention in both sets was again on the thought of keeping his attention on the black target behind the hammer.

First day, ears open:

1. 10 trials—W = 8, P = 0, I = 0.
2. 10 trials—W = 4, P = 1, I = 0.
3. 10 trials—W = 7, P = 0, I = 0.
4. 10 trials—W = 5, P = 1, I = 0.
5. 10 trials—W = 6, P = 1, I = 0.
6. 10 trials—W = 3, P = 0, I = 0.

First day, ears closed:

1. 10 trials—W = 0, P = 0, I = 4.
2. 10 trials—W = 1, P = 6, I = 3.
3. 10 trials—W = 0, P = 3, I = 7.
4. 10 trials—W = 0, P = 0, I = 10.
5. 10 trials—W = 3, P = 6, I = 1.
6. 10 trials—W = 3, P = 3, I = 1.

Last day, ears open:

1. 20 trials—W = 4, P = 2, I = 0.
2. 20 trials—W = 5, P = 2, I = 0.
3. 20 trials—W = 1, P = 5, I = 1.

Last day, ears closed:

1. 20 trials—W = 0, P = 1, I = 18.
2. 20 trials—W = 1, P = 2, I = 15.
3. 20 trials—W = 0, P = 6, I = 12.

It is evident that the sound of the hammer striking against

the glass has been an important element in the preceding reactions.

At this point in the experiments the head rest was removed and during the rest of the investigation the subject sat free but with his face close to the glass as before.

The conditions of the series just described were now altered by the introduction of another element. The subject made his muscles tense leaning slightly forward in his chair and grasping his knees firmly with his hands. The result, however, did not differ greatly from the preceding. The reactions for the last day were—

Ears open:

1. 20 trials—W = 0, P = 8, I = 9.
2. 20 trials—W = 0, P = 8, I = 11.
3. 20 trials—W = 1, P = 12, I = 1.

Ears closed:

1. 20 trials—W = 3, P = 0, I = 15.
2. 20 trials—W = 0, P = 1, I = 18.

The subject had a feeling during this series that the muscles of the arms and legs were too far removed from the eyes to make their contraction effective in inhibiting the wink and so this gave way to contraction of the facial muscles and those of the neck but the reactions continued about the same.

The contraction was then extended to the muscles of the head and scalp. The effort was so great as to throw the face into a quiver of distortion. At the same time a control series was introduced in which effort was directed merely to inhibiting, but without any unusual muscular tension. The following are the reactions for the last of five days' practice.

Ears open:

1. 20 trials—W = 0, P = 3, I = 17.
2. 20 trials—W = 0, P = 1, I = 19.
3. 20 trials—W = 0, P = 2, I = 18.

Ears closed:

1. 20 trials—W = 0, P = 2, I = 17.
2. 20 trials—W = 0, P = 3, I = 17.

Three "control" series taken on the last three days gave

1. 20 trials—W = 0, P = 18, I = 0.
2. 20 trials—W = 0, P = 8, I = 10.
3. 20 trials—W = 3, P = 1, I = 0.

It is evident from these results that contraction of the muscles of the scalp together with the other muscles of the head greatly increased the power to inhibit. This means of control was not only the most effective but it also retained its influence longer than the others. In time, however, it, too, lost its hold but as its effect lessened, the general power to control

without the aid of any muscular contraction seemed to increase. As this improvement had not been observed while the practice was confined to attempts to inhibit directly, it seemed worth while to try to determine how much time would be needed for learning to control this reflex through adjacent muscles, and the experiments directed toward this will be discussed later.

A series in which contraction of the face and scalp muscles was alternated with some in which the hands were tightly clinched clearly showed the greater effectiveness of the former in facilitating control, and finally the effect of slightly bending the head forward without any intentional muscular contraction was tried. This gave a feeling of security on account of the overlapping eye brows and the position necessarily occasioned some contraction which doubtless aided in the control. The result was much the same as when the face and scalp muscles were contracted.

In order to test the effect of distraction of the attention without muscular contraction, a series was taken in which the subject added columns of figures on a card behind the hammer. These were alternated with others in which there was only effort to inhibit. In order that the effect of previous practice in inhibition through muscular contraction might be eliminated a new subject was taken for this series. The results failed to show any effect of this kind of distraction on the wink reaction. The experiments were continued for twenty days.

Having found that we may learn to control the reflex wink by beginning with muscle situated near the eyes and gradually getting hold, as it were, of the eye muscles themselves, the question suggested itself as to whether this control could be gotten in a short time or whether the long and tedious training that our subject went through is necessary. Eight subjects in all were tested. Of these two succeeded after the first or second excitation and repeated trials failed to bring any noticeable reaction afterward.¹ Two were unable to learn to inhibit within a reasonable time, *i. e.*, two or three weeks, and the progress of the others is shown below.

The instructions were necessarily rather general. They were to contract the facial muscles and then to try by elimination to get control of just such muscles as proved effective in the inhibition.

Subject A: First series, Dec. 5.

20 trials—W = 4, P = 5, I = 3.

Subject A: Final series, Dec. 13.

20 trials—W = 0, P = 4, I = 16.

Subject B: First series, Dec. 4.

20 trials—W = 1, P = 15, I = 2.

¹*Cf.* also the experiments of Partridge (17.)

- Subject B: Final series, Dec. 9.
 20 trials— $W = 0$, $P = 1$, $I = 18$.
- Subject C: First series, Jan. 7.
 20 trials— $W = 1$, $P = 12$, $I = 7$.
- Subject C: Final series, Jan. 11.
 20 trials— $W = 0$, $P = 0$, $I = 20$.
- Subject D: First series, Dec. 30.
 20 trials— $W = 6$, $P = 5$, $I = 0$.
- Subject D: Final series, Jan. 3.
 20 trials— $W = 1$, $P = 5$, $I = 14$.

The evidence of those who succeeded was that they began with mass muscular control and gradually worked down to the essential ones by "getting their feeling." The time required was about a week. Two succeeded in five days. At the last test there was no muscular contraction noticeable to the experimenter and the subjects themselves felt no more eye tension, as they said, than would be necessary to observe a fine point at a little distance.

That this control is not due to distraction of the attention was shown by a further experiment with D immediately after his final test. He read aloud rapidly while the hammer struck against the glass as before, holding his book in such a position as to bring the hammer within the margin of the field of vision. The result was

20 trials— $W = 3$, $P = 8$, $I = 0$.

We may then summarize the results of this part of the investigation as follows:

1. The eye reflex is a complex reaction resulting from a combination of visual and auditory sensations and the final effect seems to be greater than the sum of the separate effects. Probably the order in which the stimuli are experienced has something to do with this.
2. Moderate fatigue does not noticeably affect the reaction.
3. Attention on inhibition only slightly reduces the number and intensity of the reactions.
4. Attention on distracting objects gives moderate and temporary results.
5. Distraction of the attention by various devices lessens the reaction but they soon wear out.
6. Adding has no appreciable effect.
7. Contraction of muscles remote from the eyes does not greatly reduce the reactions.
8. Contraction of muscles near the eyes has decided effect.
9. Closing the ears markedly lessens the reaction.
10. The training in control gotten through contraction of the muscles near the eye seems to have an effect on the simple

inhibitory control (*i. e.*, that in which the attention is on inhibiting), increasing its efficiency up to a certain point beyond which a great deal of practice does not carry it.

While the experiments that we have just described were in progress one of the subjects put his twenty-five weeks old baby-girl before the apparatus, the effect of which he had been trying for weeks to learn to resist but with only partial success, and to the surprise of the observers her eyes were wholly unaffected though her face was close to the plate glass, against which the hammer was striking with so much noise that most adults could learn to restrain the wink only after considerable practice, and she was looking directly across the hammer's course. This occurred at the end of the university year, and fourteen weeks later, when the close of the vacation permitted another test, the child winked every time the hammer struck. After two or three blows she looked frightened and turned to her mother, as though about to cry. During the previous test she had been undisturbed mentally as well as reflexively.

These observations suggested enlarging the investigation to include the genesis of the reflex wink, and through the kindness of two sets of parents in the university circle the writer was able to make careful tests of the matter upon two young babies.

For the first series of experiments a baby-girl was the subject. The tests were made during the middle of the afternoon. The conditions of sleep and health were noted when necessary. Tests were begun on her 68th day. On that day I made forty rapid passes with my index finger toward her eyes, almost touching them, but she did not wink. Neither did she when her eye lashes were touched with a handkerchief but if the conjunctiva was touched she usually winked, though not always, and the winks were chiefly confined to a "fresh eye," *i. e.*, she soon became accustomed to it and ceased to respond. This could hardly have been a matter of fatigue of the sense organ or nerves since care was taken to avoid this and the failure to respond was noticed after two or three winks.

Before making the tests with the apparatus we passed a brass spring about the size of the hammer back and forth in front of her, at a distance of two feet from her face, and found that she followed it with her eyes in whatever direction we held it.

Care was taken throughout the investigation to guard against fatigue and, with a baby, it is not difficult to determine when this condition is being approached.

Two sets of experiments, each of twenty trials, were then made with the hammer. In these the auditory and visual excitations were combined, as in the case of the adults. The baby was held so that her face was close to the glass with her

eyes on a level with the hammer as it struck against the other side. The force of the blow was sufficient to give one quite an unpleasant shock.

In the first set of twenty tests she winked five times and in the second three. But they were not merely winks. The baby started or jumped slightly and the wink was part of this general reaction. This will be found to play an important rôle in the development of this reflex.

On her 75th day the experiment was varied so that we might observe the effect of sudden visual excitation. A piece of soft cloth was folded several times and held between the handle of the hammer and the body of the apparatus so as largely to eliminate the sound of the hammer's blow. This rather primitive method was adopted because after trying several more elaborate plans it was found the best. The problem was to have the hammer fly up with its full force directly against the glass but to strike it so lightly as not to have the sound a disturbing element. To do this it was necessary to stop it suddenly, just before it struck the glass, and the cloth padding did this fairly well. There was usually a little noise but apparently not enough to be a disturbance.

As before, the baby's face was brought close to the plate glass with her eyes on a level with the hammer as it flew toward her. Whenever her eyes wandered from the direction of the hammer the experimenter waited until he could get her attention again or, sometimes, her father walked about with her so as to avoid fatigue and keep conditions as natural as possible.

In the first twenty tests there were two winks but they seemed entirely natural and not caused by the hammer. In order that there might be no mistake about this, one hundred and fifteen additional tests were made in sets of about twenty each, and in no instance was there the slightest trace of a wink except such as occurred between the intervals of the stimulation, and care was taken that the hammer strokes should not follow one another so rapidly as to cause any confusion here.

The experiment of touching her eye lashes with the corner of a handkerchief was repeated and she winked in about seventy-five per cent. of the cases.

On her 82nd day we began three sorts of tests all of which were made each time during the remainder of the investigation. The auditory and visual combined and the visual alone have just been described. In addition we tried the effect of sound alone. In this the baby was held so that her eyes were turned away from the apparatus while one ear was toward the glass and near it.

For convenience I will designate these three sorts of tests as "auditory—visual," "visual," and "auditory."

The visual tests were always made first so as to avoid any possible nervous effect of the noise from the hammer.

82nd day. I. Visual. (Sound largely eliminated by padding.)

- | | | | | | | |
|----|----------|---|---|---|---|----------|
| 1. | 25 tests | . | . | . | . | 3 winks. |
| 2. | 25 " | . | . | . | . | 0 " |
| 3. | 25 " | . | . | . | . | 6 " |

While some of these winks seemed natural, this was not true of them all, and it was evident that she was gaining in this reflex.

II. Auditory.

- | | | | | | | |
|----|----------|---|---|---|---|----------|
| 1. | 25 tests | . | . | . | . | 6 winks. |
|----|----------|---|---|---|---|----------|

Several of these seemed natural, but with one or two of them the start, which at first characterized every response, was noticeable. Her general reaction, however, had greatly decreased.

III. Auditory—visual.

- | | | | | | | |
|----|----------|---|---|---|---|-----------|
| 1. | 20 tests | . | . | . | . | 11 winks. |
|----|----------|---|---|---|---|-----------|

She looked frightened and at times seemed on the point of crying, quite in contrast to her behavior two weeks before, on her 68th day, when her response was more organic than selective.

89th day. She had slept very little during the day and was nervous and fretful both before and during the tests. This explains the irregularity of the results. They are interesting in showing her reaction during sickness but should not be regarded as showing her natural reflex response at this time.

I. Visual.

- | | | | | | |
|----|-----------|---|---|---|--|
| 1. | 20 trials | . | . | . | 3 winks, but only one of these seemed to be caused by the excitation. The other two had every appearance of being natural. |
|----|-----------|---|---|---|--|

- | | | | | | |
|----|-----------|---|---|---|----------|
| 2. | 20 trials | . | . | . | 6 winks. |
|----|-----------|---|---|---|----------|

3. 20 trials . . . 0 winks. During this series she was looking directly across the path of the hammer at a little girl in a red dress who was standing at the lower end of the table. Her attention being taken up in this way probably prevented her from winking, notwithstanding the other favorable conditions.

II. Auditory.

- | | | | | | |
|----|-----------|---|---|---|-----------|
| 1. | 20 trials | . | . | . | 17 winks. |
| 2. | 20 trials | . | . | . | 8 winks. |

In the second series the first five winks began with the second excitation and followed one another in succession. After that she skipped one, then winked, while the other came toward the last.

III. Auditory—visual.

1. 20 trials . . . 15 winks.

Toward the last she seemed bored but not frightened.

96th day. I. Visual.

1. 20 trials . . . 5 winks and two of these were surely natural.

2. 20 trials . . . 5 winks and one was apparently natural.

3. 20 trials . . . 2 winks but both were evidently natural.

4. 20 trials . . . 13 winks. Only one of these seemed natural but she was looking straight at the hammer and made wink after wink, consecutively, while looking at it. The experimenter took care not to have the stimuli follow one another so fast as to have a summation of effect.

II. Auditory.

1. 20 trials . . . 6 winks. Four of these appeared to be natural.

III. Auditory—visual.

1. 20 trials . . . 7 winks.

2. 20 trials . . . 6 winks but only two seemed to be caused by the apparatus.

103d day. I. Visual.

1. 20 trials . . . 2 winks.

2. 20 trials . . . 4 winks.

3. 20 trials . . . 1 wink.

4. 20 trials . . . 1 wink.

So far as we could judge all of these were responses to the excitation.

In order to see the effect of a larger object we then held her behind a glass door, with her face toward the glass, and struck the opposite side with a black cap.

1. 20 trials . . . 1 wink.

2. 20 trials . . . 1 wink.

II. Auditory.

1. 20 trials . . . 8 winks.

Here a variation was introduced. A piece of card board was tacked to the apparatus in front of the hammer so that she could be held facing it, as in the visual series, but could not see the hammer.

1. 20 trials . . . 17 winks.

2. 20 trials . . . 13 winks.

We concluded, however, that the increased number of winks was an effect of the louder and sharper noise that the card caused the hammer to make rather than the result of facing the apparatus.

III. Auditory—visual.

1. 20 trials . . . 14 winks.
2. 20 trials . . . 11 winks.

Most of the winks in the second series were toward the beginning.

Whenever her attention was taken by discomfort caused by her position she did not wink

110th day.

The tests to-day were not altogether satisfactory because the baby was so restless that it was hard to keep her attention. She was not irritable but was more than usually taken up with herself and the objects around her. She seemed more interested in the experimenter than in the experiments.

I. Visual.

1. 20 trials . . . 10 winks.

Two of these came late and one was undoubtedly natural.

2. 20 trials . . . 9 winks.
3. 20 trials . . . 9 winks.

II. Auditory:

1. 20 trials . . . 3 winks.

All of these came with the start that has characterized all these reactions at the beginning.

We repeated the variation, introduced on her 103d day, of putting a piece of card board in front of the hammer so that she could face the apparatus.

1. 20 trials . . . 9 winks.

Three of these were only half winks but they seemed to be responses to the noise.

2. 20 trials . . . 6 winks.

Four of these were very slow.

III. Auditory—visual.

1. 20 trials . . . 5 winks.

Sucking her fingers seemed to take her attention during this series.

2. 20 trials . . . 7 winks.

117th day. I. Visual.

1. 20 trials . . . 17 winks.
2. 20 trials . . . 12 winks.
3. 20 trials . . . 11 winks.

During the latter part of the third set it was evident that her attention was taken up by something beyond the hammer, and as a result she did not wink.

4. 20 trials . . . 10 winks.

Here, again, she seemed to be looking at some object.

II. Auditory.

1. 20 trials . . . 11 winks.

In addition to these there were four partial winks.

2. 20 trials . . . 4 winks.

During the first part of this series her attention was taken up with trying to do something, and she did not begin to wink until she found that she could not accomplish it and so released her attention.

3. 20 trials . . . 8 winks.

Besides these there were four partial ones. Her attention was again occupied during a part of the time and then she did not wink.

The start which characterized her reaction to auditory stimuli at the beginning has been occurring less frequently and to-day was not noticeable.

With the piece of card board in front of the hammer and the baby again facing it we found

1. 20 trials . . . 18 winks.

2. 20 trials . . . 14 winks.

In the second set there were also three partial winks.

III. Auditory—visual.

1. 20 trials . . . 18 winks.

She also moved her eye lids, as though about to react, the remaining two times but did not finish the wink.

2. 20 trials . . . 16 winks.

In addition to these there were four partial winks.

126th day. I. Visual.

1. 20 trials . . . 11 winks.

2. 20 trials . . . 14 winks.

3. 20 trials . . . 13 winks.

Delayed winks were quite noticeable in these series.

II. Auditory.

1. 20 trials . . . 5 winks.

2. 20 trials . . . 7 winks.

The card board variation, again, gave the following.

1. 20 trials . . . 9 winks.

There were also two or three quivers.

2. 20 trials . . . 8 winks.

Besides these, two quivers were noticed but during this series the card board attached to the apparatus seemed to take her attention.

3. 20 trials . . . 4 winks.

III. Auditory—visual.

1. 20 trials . . . 12 winks.

2. 20 trials . . . 14 winks.

In addition to these there were four partial winks. The other two times not a muscle moved.

3. 20 trials . . . 13 or 14 winks.

The uncertainty here arose because we were unable to determine whether one was natural or not.

In an investigation of this sort there is much that figures cannot show. The general organic reaction must be a matter of observation, and I find in my note for this day, "she is evidently becoming less affected by auditory stimuli while her reaction to visual excitations is continually increasing." In reference to this her parents said, also, that she had lately begun to notice everything in the room.

131st day. I. Visual.

1. 20 trials . . . 16 winks.

In addition there was one partial wink.

2. 20 trials . . . 15 winks.

An organic response, somewhat similar to that noticed at an earlier age in connection with the auditory excitations, was observed at about this time in the visual reactions. She sometimes winked with great force, shutting her eyes together tightly, as though the visual excitation disturbed her. This sort of a response to auditory stimuli had long since entirely disappeared.

II. Auditory.

1. 20 trials . . . 3 winks.

2. 20 trials . . . 7 winks.

3. 20 trials . . . 5 winks.

With the card board in front of the hammer.

1. 20 trials . . . 3 winks.

2. 20 trials . . . 5 winks.

III. Auditory—visual.

1. 20 trials . . . 8 winks.

Several times in this series, when she was looking directly at the hammer, she winked fiercely.

2. 20 trials . . . 13 winks.

3. 20 trials . . . 12 winks.

In the last two series she winked whenever she was looking right across the hammer's path and, apparently, at nothing in particular.

141st day. I. Visual.

1. 20 trials . . . 10 winks.

2. 20 trials . . . 11 winks.

3. 20 trials . . . 10 winks.

II. Auditory.

1. 20 trials . . . 3 winks.

2. 20 trials . . . 0 winks.

3. 20 trials . . . 2 or 3 winks.

4. 20 trials . . . 0 winks.

III. Auditory—visual.

1. 20 trials . . . 20 winks.
2. 20 trials . . . 20 winks.
3. 20 trials . . . 19 winks.

The organic response reached its culmination in the auditory—visual to-day. In this series she frequently shrank back and looked frightened, closing her eyes and keeping them closed for several seconds as though the stimulus pained her.

152nd day. I. Visual.

1. 20 trials . . . 10 winks.
2. 20 trials . . . 13 winks.

She seemed somewhat more sensitive than usual in this series.

3. 20 trials . . . 6 or 7 winks.
4. 20 trials . . . 5 winks.
5. 20 trials . . . 5 winks.

II. Auditory.

1. 20 trials . . . 7 winks.
2. 20 trials . . . 7 winks.
3. 20 trials . . . 0 winks.
4. 20 trials . . . 0 winks.
5. 20 trials . . . 0 winks.

III. Auditory—visual.

1. 20 trials . . . 19 winks.
2. 20 trials . . . 20 winks.
3. 20 trials . . . 18 winks.
4. 20 trials . . . 19 winks.

The second subject studied was a baby boy. In his case the investigation was begun at a little earlier stage.

As before, a careful test was made to be sure that he could see an object the size of the hammer at a considerably greater distance than that at which the hammer would approach him.

46th day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.
3. 20 trials . . . 0 winks.
4. 20 trials . . . 0 winks.
5. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 20 winks.
2. 20 trials . . . 20 winks.
3. 20 trials . . . 20 winks.
4. 20 trials . . . 20 winks.
5. 20 trials . . . 20 winks.

I have called his reactions to the auditory stimulus winks

but that does not characterize them properly. At every stroke of the hammer he shrank back, much as a small boy does when one makes rapid passes at his face with the hand. And then, in addition, he shut his eyes tightly as though the sound were painful. Still he did not jump, at least not perceptibly. The latter mode of reacting seems to be a later stage of the organic response. (*Cf.* also p. 244.)

It was noticed that after twenty excitations, he sometimes seemed to become accustomed to the stimulus and ceased to react.

53d day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 15 or 16 winks.
2. 20 trials . . . 17 winks.

The same shrinking in response to the auditory stimuli was observed only it was a little less marked. The so-called winks are not yet that but rather the participation of the eyes in the general organic shrinking reaction.

60th day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.

These series were unusually successful in the fact that he looked directly through the glass across the field of the hammer all the time. For this reason delays, efforts to get his attention, were unnecessary.

II. Auditory.

1. 20 trials . . . 6 winks.
2. 20 trials . . . 8 winks.

The shrinking, hitherto so noticeable, was hardly apparent to-day. He paid little attention to the sound except for the winking reaction which now for the first time took the place of the shrinking. This was also the first day when he did not act as if the stimulus pained him or as though he wanted to cry.

67th day. I. Visual.

1. 20 trials . . . 0 winks.
2. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 9 winks.
2. 20 trials . . . 8 winks.

One or two of the winks in the last series may have been natural.

The shrinking reaction was entirely absent and there was no evidence whatever that the sound disturbed him; indeed, to-

day seemed to mark a turning point in his psychic life, since, for the first time, he turned his head to see what was making all the noise.

74th day. I. Visual.

1. 20 trials . . . 0 winks.

One occurred but it seemed perfectly natural.

2. 20 trials . . . 0 winks.

II. Auditory.

1. 20 trials . . . 15 winks.

2. 20 trials . . . 3 winks.

In the last series his brow contracted seventeen times but in only in the three cases did it go on to the wink. There were thus seventeen reactions to the excitation but they were of varying degree. This may indicate a transitional stage.

3. 20 trials . . . 19 winks.

83d day. I. Visual.

1. 20 trials . . . 0 winks.

2. 20 trials . . . 2 winks.

3. 20 trials . . . 2 winks.

II. Auditory.

1. 20 trials . . . 17 winks.

2. 20 trials . . . 15 winks.

87th day. I. Visual.

1. 20 trials . . . 5 winks.

Three of these were certainly caused by the apparatus, the other two were uncertain.

2. 20 trials . . . 4 winks.

One was accompanied by the characteristic reflex "start" which occurred now for the first time in the visual reactions. (See p. 241, *cf.* also pp. 244 and 245.)

3. 20 trials . . . 2 winks.

4. 20 trials . . . 5 winks.

One or two in the last series seemed natural.

5. 20 trials . . . 5 winks.

II. Auditory.

1. 20 trials . . . 8 winks.

2. 20 trials . . . 6 winks.

On his 71st day his father found that he winked occasionally when he thrust his finger at his eyes but to-day he reacts every time to this stimulus.

It is interesting, in connection with the development of the reflex wink, to learn that on his 86th day, just when the visual reflex was making its most rapid development, the child suddenly burst into a violent fit of crying and hid his face in his mother's lap at the sight of a neighbor's baby.

Unfortunately the experiments were brought to an end at

this time by the baby being taken out of town and it was not possible to test him further until his 130th day.

130th day. I. Visual.

1. 20 trials . . . 5 winks.

In addition to these there were two or three partial reactions, *i. e.*, slight contraction of the brow with noticeable movement of the eyes.

2. 20 trials . . . 12 winks.

II. Auditory.

1. 20 trials . . . 15 winks.

2. 20 trials . . . 13 winks.

I am indebted to Superintendent Harold Barnes for the following note regarding another of his boys. At the end of the baby's eighth week his father noticed that he did not wink however suddenly an object might be brought close to his eyes.¹

Miss Shinn (19) tells us, too, that her niece winked reflexively for the first time on her fifty-sixth day, when a head was suddenly thrust close to her face.

The following general conclusions may be drawn from this investigation.

1. Until about the fiftieth day babies are excessively sensitive to auditory stimuli.

2. The first response to a sudden sharp sound is organic. It is a general bodily contraction, which may be accompanied by a jump, but if not, it always develops soon into this mode of response, and in this general reaction the eyes participate. The experiments on the baby-girl, the first subject, were evidently begun at about the close of this period. Later, as this investigation shows (and in the case of the little boy, the second subject, it was about the sixtieth day), the bodily response ceases and the wink becomes differentiated as a distinct reaction.

3. The visual reflex does not appear until much later. In the two cases investigated it was first observed shortly after the eightieth day.

4. Sensitiveness to auditory and visual stimuli pursue an inverse development. As the auditory reaction becomes less marked the visual appears and gradually increases in frequency until the adult condition is approached, in which the visual response is common and the auditory rare.

5. Reflex reaction, so far as the wink may be regarded as

¹Preyer (16) found that his child winked at the quick approach of an object to his face on his sixtieth day. He also noticed the slower closing of the lids than in adults but speaks of it only up to the twelfth day whereas in this investigation it was observed at times in the baby-girl up to her one hundred and twenty-sixth day.

typical, is evidently inherited in the race but learned by the individual. The elements from which the reactions can be built up are given in heredity, but the nicety of their adaptation to ends must be learned.

The manner in which the first subject reacted to the auditory—visual stimulus on her 82nd day and the greater number of winks usually caused by this double excitation is interesting. Can it be that at this early day sight of an approaching object gives new significance to the noise that immediately follows? Her attitude toward it on that day indicated a fear wholly absent in the auditory reactions that just preceded.

The auditory and visual tests were also made on another baby-boy on his 265th day. As the earlier experiments had indicated that withdrawal of the attention from the excitation greatly alters the reaction, two sets of visual tests were taken in alternate series, one such as we have already described while the other differed in having a lighted match held behind the hammer so that, as the baby looked across its path, his attention would be taken by the light. The results are given in the order in which the experiments were made.

265th day. I. Visual.

20 trials . . . 14 winks.

Visual with lighted match.

11 trials . . . 3 winks.

The first seemed to be a natural wink.

Visual.

19 trials . . . 19 winks.

Visual with lighted match.

20 trials . . . 6 winks.

Two of these seemed caused by the sudden appearance of the light.

Visual.

20 trials . . . 10 winks.

Visual with lighted match.

18 trials . . . 3 winks.

One was so gentle that it seemed natural.

In the series with the lighted match the winks came late in the series, usually at the last.

II. Auditory.

20 trials . . . 0 winks.

These experiments were repeated on his 296th day but the match did not hold his attention as before. Whenever his eyes rested on it, however, or on the experimenter, who stood behind the hammer, the reactions were less frequent than at other times.

In order to find out the part played by sound and sight in

the reflex wink of older persons, experiments were made on four university students and on a five year old boy.

In order that the sound might be reduced as much as possible the ears of the subjects were packed with cotton which was kept in place by a bandage passing under the chin. This was

TABLE.

	A						B			C				D					E			
	C			A			C	V	A	X	C	V	A	X	C	V	A	X	C	V	A	X
	W	W ¹	W ²	W ³	W ⁴	O	6	4	1	0	0	0	0	0	0	0	0	0	0	0	0	0
W ¹	4	1	0	0	0	0	7	0	0	0	2	0	0	0	17	0	0	0	17	0	4	4
W ²	5	4	0	1	0	0	7	0	0	0	4	0	0	0	3	0	0	0	2	1	1	2
W ³	5	4	0	1	0	0	2	1	0	0	10	0	1	6	0	0	0	3	1	0	1	3
W ⁴	5	5	6	9	2	2	4	0	2	4	4	0	0	4	0	8	0	2	0	1	7	2
W ⁴	0	7	4	5	8	0	0	0	0	0	0	0	6	1	0	0	0	0	0	0	4	0
O	0	10	10	5	16	19	0	19	10	16	0	20	11	9	0	11	0	15	0	18	3	9

C = control, *i. e.*, auditory and visual with no attempt to isolate any element in either.

V = visual.

A = auditory.

X = auditory with eyes turned toward the apparatus but hammer covered.

W = full wink, W¹, W², W³, W⁴ = lesser degrees of winking, W⁴ indicating the slightest noticeable twitch of the eyes.

O = no wink.

All of the subjects were university students except "E" who was a boy of five and a half years. V¹ in "D" gives the results of a further attempt to eliminate sound by having an assistant press the bandages close to the ears of the subject.

in addition to the cloth bumper that broke the force of the hammer as it was about to strike the glass.

The sound could not be entirely eliminated, but this arrangement so muffled it as largely, if not wholly, to neutralize its influence on the reactions.

It is evident from the table that the reaction caused by excitation of the wink reflex, when visual and auditory elements are combined, is the result of both factors, and the part played by each varies with different individuals. In "A" the auditory stimulation was less effective, with "B" they did not differ greatly while in the others the reactions to the auditory was more marked.

Partridge (17), with a similar apparatus, in a series of experiments on children from five to fifteen years of age, found a gradual improvement in control with increasing age, and these results show that Dr. Sanford¹ was right in ascribing much of the effect to the sound of the hammer and to the rattle of the apparatus. Further, the tests on the five and a half year old boy, 'E,' do not indicate any essential difference in the reactions after that age.

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CORRESPONDENCE.

MADISON, WIS., April 2d, 1903.

To the Editors of the *American Journal of Psychology*:

Gentlemen:—

I am much interested in Professor Titchener's plea for summaries and indexes in connection with psychological papers. I entirely agree with him as to the very special value of the summary, no matter whether the paper be short or long. I cannot at all agree with him, however, as to the necessity for long papers in presenting experimental results. Professor Sanford is certainly moderate in his estimate that in most cases, where one hundred pages are written, the same statement could have been condensed and better stated in twenty-five pages. Professor Titchener asks for a special index for all papers exceeding twenty-five pages in length. The essential point is, it appears to me, that papers should not exceed twenty-five pages in length. With a few obvious exceptions, it may be stated with some emphasis that a paper recounting the result of an ordinary research can, with skill in presentation, be confined to this limit. Professor Titchener tells us that long papers are inevitable, and are an evidence of an advance in psychology. Unquestionably the researches thus recorded are signs of advance, but the papers themselves are frequently an evidence of the inability of the investigator to use language or to arrange his thoughts economically. My own solution for the difficulty would be to insist that the essential parts of the paper, together with the interpretation of the points presented, be stated concisely and forcibly; furthermore, that all details by which the evidence is enforced, and the raw material out of which the conclusions have been drawn, shall again be systematically arranged in a series of appendices. The fault with long papers is largely the promiscuous mingling of all sorts of material, which may have been essential in the conducting of the research, but is not essential to the statement of its results. It is much to the credit of American psychology that it has to such a large extent avoided the undesirable habit of long papers, which have come to be characteristic of many schools of psychological writers.

Believe me, very truly yours,

JOSEPH JASTROW.

I am very glad to find my recommendations of summaries and tables of contents and indexes thus heartily endorsed by Professor Sanford and Professor Jastrow. If our three laboratories will henceforth systematically set a good example in these respects, I have no doubt that others will follow it.

I am glad, also, that the question of length of papers has been brought into open discussion. Personally, I have for some time felt that our magazine articles in general are getting to be too long. I tried to put this feeling into practice by cutting down my paper in the Wundt *Festschrift* (with 16 figures in the text) to 25 pp. Apparently the feeling is shared by other American psychologists: for I find that, in the same *Festschrift*, Angell takes only 22 pp., Cattell 6, Judd 17, Pace 15, Scripture 17 and Stratton 25.

At the same time, I am sure that the question is much less simple than Professor Jastrow makes it. Neither the 25 pp. limit nor the plan of appendices will work in every case. Take Angell's paper on sound intensities, in *Phil. Stud.*, vii. It fills 55 pp.; and I fail to see either that it could have been further condensed (it is rather over-condensed already) or that anything at all would have been gained by relegation of parts of it to an appendix. Besides, all men do not write in the same way. Consider the relative length of the sentences in the work of Lipps and of Meinong. Both men have a good deal to say; each says it best in his own way; it would be absurd to run both into the same mould. And I am convinced, also, that one reason for the increasing length of papers is, really, that writers nowadays have more psychological material; the intrinsic quality of the output is, perhaps, no better than it was at first, but we have better methods, and get more facts.

We must remember, too, that perhaps in the majority of cases the longest papers are theses, first attempts at serious writing. The editor's duties in such a matter are very delicate. We have a protectorate, which must in some way be reconciled with independence of the writers. We can correct, advise, suggest, set an example; we can hardly do more without infringing on the author's rights, or at least hurting his feelings.

On the whole, therefore, I think it unwise to set a definite limit to the length of papers or to prescribe a plan for their arrangement. I want every one who has something to say to say it, and to say it as he likes best: only I ask that he give the reader such mechanical aids to the comprehension of his writing as he readily can. Further editorial legislation, such as Professor Jastrow suggests, would cripple many young investigators, who learn the better path by experience. You cannot create style by act of parliament. But, I may repeat, this editorial conservatism does not affect my belief that most of the things we read might, as a matter of fact, have been considerably shortened.

E. B. TITCHENER.

LITERATURE

Agnosticism. By ROBERT FLINT, D. D., LL. D., F. R. S. E., Professor in the University of Edinburgh. New York, Chas. Scribner's Sons, 1903. pp. xviii, 664. Price, \$2.00.

"The present volume," Professor Flint tells us, "is part of what was many years ago announced as meant to form when completed a System of Natural Theology which would deal with four great problems." The first problem, that of evidence for belief in the existence of God, was dealt with in his *Theism*; the second, the refutation of antitheistic theories, was partly—as regards atheism, materialism, positivism, secularism, pessimism, and pantheism—taken up in his *Anti-theistic Theories*, and is, as regards agnosticism, taken up in the present volume. The two remaining problems, the delineation of the character of God, as disclosed by nature, mind and history, and the tracing of the rise and development of the idea of God, have been treated only so far as they come under discussion in the article *Theism*, in the 9th. edn. of the *Britannica*.

The present work, then, is not an historical, colorless estimate of the part played by Agnosticism in modern thought; it is a partisan work, destined to refute the agnostic. Let us see how it opens.

Professor Flint begins by giving Hutton's and Huxley's accounts of the coinage of the term. According to Hutton, Huxley suggested the word *before* the formation of the Metaphysical Society, taking it from St. Paul's mention of the altar "to the unknown God." According to Huxley, it was coined *after* the formation of the Metaphysical Society, "as suggestively antagonistic to the 'gnostic' of Church History." Now one of these accounts must be false if the other is true. Professor Flint remarks that both "well deserve to be borne in mind," and appears to think that both alike represent the facts. Possibly they do; possibly the term is of mixed associative origin. Nevertheless, the reader is not reassured by finding that the author, in his haste to enter upon detailed destructive criticism, allows the discrepancy of the two statements entirely to escape his notice.

Professor Flint next revives the etymological argument. "It was contrary to Greek usage to terminate with *ikos* a word which commenced with *alpha privativum*." The fact is, that it was contrary to classical Greek usage to terminate with *ikos* a word so beginning, in which the negative meaning was retained. If the negative meaning dropped out, and the word became positive, the formation was allowable: witness the adjective *aoristikos*. In so far, then, as agnosticism represents a positive and not merely a negative tendency (as it surely must, if it takes 664 pages for its refutation), the most classical of ancient Greeks would have had no objection to the word. But, Greek philology apart, what does the argument matter? If the genius of the English language allows and has for a long time past allowed such formations, why need the modern Englishman trouble about the ancient Greek? If Professor Flint will consult the *A* in any historical English dictionary, he may perhaps be induced to withdraw this part of his indictment.

We are next told that Huxley had better have called himself a 'sceptic,'—'sceptic' being a good old philosophical word that meant

the same thing as 'agnostic.' True, "it must be admitted that it has acquired an offensive connotation." But is a man bound to call himself by a name that has acquired an offensive connotation? Granted that 'agnostic' has acquired just the same connotation—a statement, however, that does not accord with the later statement that "the man who calls himself an agnostic implicitly claims to be no common man, but a philosopher"—granted this: may not a man coin a word to get a breathing-space for himself and his opinions in the push and hustle of orthodoxy?

"The criticism in which I have thus far indulged may seem to some of my readers rather hypercritical." Let us hope so, if we hope for any fairmindedness in the reading public. But stranger things are to come. We find Professor Flint deciding, with his eyes open, that he cannot meet agnosticism on its own or indeed on any common ground. "It is in vain for a non-agnostic [a pretty term, if we are considering terms!] to seek to find a definition of agnosticism which will satisfy an agnostic. Any definition of agnosticism which will satisfy an agnostic must of necessity fail to satisfy a non-agnostic." We are taught in science that the definition of a standpoint or movement or attitude should come after the standpoint or movement or attitude has been thoroughly canvassed, in its historical bearings; the definition should be inductive and impartial, and may obtain whether we accept or reject the mode of thought defined. Not so, it would appear, in a System of Natural Theology! First of all, you have the right to define a movement at the outset to suit your own ideas of it. Secondly, it is true, you have the duty of not "judging any system merely by the definition" that you give. But—either your definition prejudices you, and so you *do* do things "unfair and unreasonable;" or your definition must be modified by your treatment of the systems, and then it has needlessly prejudiced your agnostic readers. It is hardly necessary to say that the 'duty,' beautiful as it looks in theory, does not affect Professor Flint's practice. He starts out with a definition which no agnostic would accept, and, at the end, finds that definition triumphantly vindicated by his discussion of the systems.

Once more, and we have finished with this analysis. Professor Flint assures us that he has no animus against Huxley. "Great Britain may well be proud to have had such a son as Thomas H. Huxley. I must reject any view of his which seems to me erroneous; but the fact of a view being his can never, I feel sure, be among my motives or reasons for rejecting it." Imagine this sort of preface to a piece of scientific criticism! One who has positive arguments to adduce need not apologize for adducing them: we are all working in the interests of truth, and are grateful enough—though we may be sore for a moment—if our clay idols are broken. But when a man comes to us with the assurance that these idols, after all, were delicately moulded and had their value as works of art, we rather suspect him of the very animus which he is at pains to deny.

We have dealt only with the first 20 pages of the 664. We have, however, probably said enough to give the reader an idea of the contents of the book. It is comprehensive, scholarly, erudite—and *ex parte* from cover to cover. For those that like their history of philosophy written in this way, this is the sort of book that they will like. The rest—we hope the majority—will use the book as a work of reference, as saying the worst that can be said against a great movement of modern thought, but will hardly trust it as a guide to the course of that movement.

P. E. WINTER.

Experimental Psychology and its Bearing upon Culture, by GEORGE MALCOLM STRATTON. The Macmillan Company, N. Y., 1903. pp. vi+331.

The author has undertaken to state the standpoint of the New Psychology, to pass in review some of the chief results already attained by it, and to assess the value of those results for culture and life. This general problem determines the subject matter treated. There is first an historical introduction; a second chapter deals with the general character of the psychological experiment; a third with the possibility of mental measurement; two with unconscious ideas; one with illusions; two with space perception; two with memory; one with imitation and suggestion. The remaining chapters indicate the application of the results of psychology to culture. There are two on æsthetics (the enjoyment of sensations and their forms; the color and differentiation of the Fine Arts), and two on the philosophical and ethical bearings of the new science. These discuss the connection of mind and body and the spiritual implications of experimental work.

The historical introduction begins with Aristotle and ends with Wundt. English empiricism, Berkeley's work in vision, the evidence of surgery in the removal of cataracts, Goethe's experiments with colored glass, the personal equation of the astronomers, Helmholtz' work in optics and sound, and the phrenologists Gall and Spurzheim are all shown to have had more or less influence upon the development of the New Psychology. The immediate ancestry of the science is, however, traced to Weber, Fechner and Wundt. Of Weber it is said (p. 10) that he not only aroused interest in his results, but also made men recognize the experimental method as a mode of procedure in psychology. This statement attributes to Weber much more than is his just due. Weber was first and last a physiologist; his work on the sensitivity of the skin and on the sensible discrimination of weights is wholly physiological in character. His theory of sensation circles (a purely physiological conception) and the absence of any psychological insight in the *Tastsinn und Gemeingefühl* are sufficient evidence for this statement. Of Fechner it is said, that he tested Weber's Law by many thousands of experiments in lifting weights; on the basis of these results, he cast Weber's Law into a mathematical formula. And, further, having satisfied his mathematical impulses, he fell to developing the philosophical implications of his formulæ (p. 12). This certainly, is an unwarranted misrepresentation of Fechner's service to experimental psychology. His place as one of the founders of the science rests upon psychological work of the first order, quite independent of psychophysics; for example his work on after-images. Even James, who has little love for psychophysics, admits that Fechner had great psychological insight. And, furthermore, Fechner's philosophical system was already developed long before he began his work on psychophysics. The *Elemente*, as a matter of fact, was undertaken to prove his philosophical theory, and not conversely.

The treatment of mental measurement is curious. It begins by showing that some mental processes can be measured. Thus, one is able to measure reaction times. One is also able to determine the amount of contrast induced by a complementary color. But the issue is again raised whether, in the instances cited, any mental process is measured. The question is further considered, in a discussion of four kinds of psychical quantity: these are intensity, temporal and spatial quantity, and simple enumeration. (i) *The quantity of intensity*. The objection so often made to Fechner's conception of intensive magnitudes, that no sensation intensity is made up of smaller intensities, is met by the reply that although an experience is indivisible it

may be none the less quantitative. Although the author seems to think that this reply meets the objection, he is inclined to believe that even if intensity should fail, there is still hope for mental measurement in the other quantities. (ii) *Spatial quantity*. The argument in this case is, that space is not a universal form of mental process; but that the object which we have in mind may be spatial, without the higher mental processes sharing in this property. As an example of this sort of 'non-physical' spatial quantity, is cited the degree of divergence produced in two parallel lines by cross-hatching. (iii) The psychologist has good grounds for assuming *temporal quantity* within his own field. (iv) The quantity of *simple enumeration* depends upon the proposition that wherever any real differences exist, the notion of quantity and number also exists. In this sense, quantity is as much a mental attribute as a physical one. From this discussion, the author concludes that "mental phenomena are quantitative." This treatment is unusual, in that it wholly disregards the development of the concept of mental measurement, in the hands of Fechner, Delbœuf, Müller, Stumpf, Wundt and Ebbinghaus.

The chapters on illusions, space perception and memory ought to do great good, not only in the way of popularizing the results of psychological investigation, but also as proof of the efficiency of the science itself. And the chapters on imitation and suggestion should be of value, as an antidote to popular superstition regarding hypnotism and spiritualism.

There are minor slips. Thus, the statement (p. 9) that Vierordt made a study of the time sense more than half a century ago, is not true; the work was published in 1868. Taken as a whole, however, the book is a serious piece of work. It should do much to remove misconceptions, and to give a proper understanding of the standpoint and results of experimental psychology. H. C. STEVENS.

Sprachgeschichte und Sprachpsychologie, von W. WUNDT. Leipzig, W. Engelmann, 1902. pp. 110. Price Mk. 2.

This work is primarily a reply to B. Delbrück's criticism of the *Völkerpsychologie* in his *Grundfragen der Sprachforschung*, 1901. It contains supplementary essays on gesture language, phonetic change, the fundamental questions of syntax and the origin of language. Especially interesting is the introductory chapter, which differentiates the Herbartian psychology, with its application of psychological norms to language, from modern psychology, which derives psychological laws from language. Interesting, too, is the proof of survival influences of the older classical philology and of romanticism upon the current science of language. The work as a whole forms a valuable addition to the discussions of the *Völkerpsychologie*.

Le mensonge: étude de psycho-physiologie pathologique et normale, par G. L. DUPRAT. Paris, F. Alcan, 1903. pp. 190. Price fr. 2.50.

M. Duprat is already well known as a writer on mental pathology, as the author of an *Éthics* which has recently been translated into English, and as the translator of Baldwin's *Social and Moral Interpretations*. His present work is based upon returns to a questionnaire regarding children's lies, but refers also to lying as it is found in uncivilized peoples, and among civilized adults, normal and abnormal. He concludes that lying is dependent upon tendencies which have their roots in character, in affective disposition, in physiological constitution and in neuro-muscular diathesis. His remedy is the instillation of "true ideas and generous sentiments," and the development of a critical sense, by scientific education.

A Philosophical Essay on Probabilities, by PIERRE SIMON, Marquis DE LAPLACE. Translated from the 6th French edition by F. W. TRUSCOTT, Professor of Germanic Languages, and F. L. EMORY, Professor of Mechanics and Applied Mathematics in the W. Virginia University. New York, J. Wiley & Sons, 1902. pp. iv, 196. Price \$2.00.

The first thing that strikes one about this book is that it has no index. The second is, that it has no notes, not even to the historical ch. xviii. And the third is that the translators have set themselves a task that is too high for them. They are evidently unfamiliar with the terms ordinarily employed in Probability; witness their use of 'hope' for 'expectation;' while at times the mere French text has proved too much for them; witness the confusion of 'sol' with 'soleil,' p. 143.

Recherches cliniques et thérapeutiques sur l'épilepsie, l'hystérie et l'idiotie, par BOURNEVILLE. Vol. xxii. Paris, F. Alcan, 1902. pp. clx, 236.

Part i. gives the yearly report (1901) of the Bicêtre and the Fondation Vallée, including an interesting memoir on schools for abnormal children in all parts of the world. Part ii.—Instructions médico-pédagogiques—shows the mode of recording and diagnosing cases received at the Bicêtre. Part iii., written by Dr. Bourneville in collaboration with MM. Boyer, Crouzon, Philippe, and others, contains clinical and therapeutical reports and suggestions, together with various notes upon pathological anatomy.

Experiments on Animals, by S. PAGET. With an Introduction by Lord Lister. The Science Series, No. II, pp. xvi, 387.

This is the second edition of a work issued in England in 1900 by the Secretary of the Association for the Advancement of Medicine by Research. It is an altogether admirable summary of the topic with which it deals. "Its earlier pages," we read in the Introduction, "deal with physiology, the main basis of all sound medicine and surgery. The examples given in this department are not numerous; they are, however, sufficiently striking, as indications that, from the discovery of the circulation of the blood onwards, our knowledge of healthy animal function has been mainly derived from experiments on animals. The chief bulk of the work is devoted to the class of investigations which are most frequent at the present day" (bacteriology, action of drugs). The concluding part of the volume discusses the Vivisection Act of 1876.

Response in the Living and Non-Living, by J. C. BOSE. London, New York and Bombay, Longmans, Green and Co., 1902. pp. xix, 199.

In this work the author has brought together and amplified the results of a series of papers, published between 1900 and 1902, the aim of which is to prove that "living response in all its diverse manifestations is found to be only a repetition of responses seen in the inorganic." He finds in animal, plant and metal the same phenomena of negative variation, the same relation between stimulus and response, the same effect of superposition of stimuli, the same fatigue effects, the same effects of stimulants, depressants and poisons! The papers referred to have been published in reputable magazines, and a part of the author's experimental work was done in the laboratory of the Royal Institution. The tone of the book is confident, even dogmatic; the illustrations are numerous and convincing.

And yet—what is the experimental basis of the conclusions? (1) Certain limited aspects of the changes produced in muscular, nervous and plant tissue by certain modes of stimulation, and (2) certain electrolytic effects appearing when moist conductors are brought into contact with metallic surfaces and these are caused to vibrate! The superficial analogy between these two classes of results (and under the former heading the results are not accurately described) is to read us the riddle of life and the mechanism of life! It may very well be that all these things "are determined . . . by the working of laws that know no change, acting equally and uniformly throughout the organic and the inorganic worlds;" at any rate, many of us hope that it is so. But knowledge is not advanced by the ignoring of large classes of facts and the application of a method of crude analogy to the rest.

More Letters of Charles Darwin: a Record of his Work in a Series of hitherto unpublished Letters. Edited by F. DARWIN and A. C. SEWARD. New York, D. Appleton & Co. 1903. Vol. I, pp. xxiv, 494; Vol. II, pp. viii, 508.

This is an extraordinarily interesting book. By the help of unpublished letters and other material not available for the *Life and Letters*, the editors have been able, with very few repetitions from the latter book, to give a practically complete account of Darwin's life work. The letters are grouped under the headings Evolution, Geographical Distribution, Man, Geology, Botany, Vivisection and Miscellaneous Subjects. Both volumes are illustrated: portraits are given of Darwin and his wife, of Romanes, F. Müller, Lyell, Forbes, Hooker, Henslow, Huxley, Gray and others. The editors are to be congratulated upon their completion of a work which will have a permanent value in the history of science.

Pure Sociology, by L. F. WARD. New York, The Macmillan Co., 1903. pp. xii, 607.

This bulky work contains the author's revision of lecture courses delivered in 1897-9 at the Universities of Chicago, West Virginia and Stanford. The writer "regards all social phenomena as *pure* which are unaffected by the purposeful efforts of man and of society itself." He consequently prints, as a sub-title: *A Treatise on the Origin and Spontaneous Development of Society*. The book falls into three parts: Taxis, which discusses the general characteristics of pure sociology, its subject-matter and methods; Genesis, which treats of the biological origin of the subjective faculties, of social mechanics, statics and dynamics, and of the social forces, ontogenetic, phylogenetic and sociogenetic; and Tesis, which deals with the biological origin of the objective faculties, the conquest of nature, and the socialization of achievement.

Les grand philosophes: Aristote. Par C. PIAT. Paris, F. Alcan, 1903. pp. viii, 396.

M. Piat is the editor of the collection entitled *Les grand philosophes*, nine volumes of which have already appeared, among them a study of Socrates from his own pen. The present work is a useful monograph on the Aristotelian system. It is divided into four books: Being (definition of first philosophy; determination of the categories; substance; the derivatives of substance; causes), Nature (movement; the unmoved mover; the heavens), Mind (mind and its faculties; nutrition; sensation; thought; desire), and Conduct (the individual; the family; the city). A Conclusion traces the course of naturalism from Plato to Aristotle, and from Aristotle to Strato. The book is fully

annotated, and contains a working bibliography. The writer is especially interested in the relation of Thomas Aquinas to Aristotle.

Les obsessions et les impulsions, par A. PITRES and E. RÉGIS. Paris, O. Doin, 1902. pp. 434.

This little work follows the regular programme of definition, description and classification, records of cases, diagnosis and treatment, and legal aspects of the disorders treated. Obsession is "un syndrome morbide caractérisé par l'apparition involontaire et anxieuse dans la conscience de sentiments et de pensées parasites qui tendent à s'imposer au *moi*, évoluent à côté de lui malgré ses efforts pour les repousser et créent ainsi une variété de dissociation psychique dont le dernier terme est le dédoublement conscient de la personnalité." "L'impulsion morbide est, dans le domaine de l'activité volontaire, la tendance impérieuse et souvent même irresistible au retour vers le pur réflexe." The authors cover a wide range of fact, and write with sanity and reserve.

La mimique, par E. CUYER. Paris, O. Doin, 1902. pp. 366.

A careful study of expressive movements, illustrated by many original drawings. The author analyzes the expressive movements of face, head, trunk, arms and legs, and gives a 'dictionary' of emotions and their expression. Curiously enough, no mention is made in his bibliography of the works of James and Wundt.

History of Philosophy, by WILLIAM TURNER. Ginn and Co., Boston, 1903. pp. 674.

The best part of this book is entitled "The Philosophy of the Christian Era." Patristic thought is hastened over and scholasticism is treated more fully. A brief account of perhaps a score of the great scholastics, which occupies two hundred pages and much of which has evidently been worked up from original sources, makes it a handbook of much practical value. No Catholic has yet given us such a compendious and valuable classbook on this field, and it is sure to make it all the more worthy.

Outlines of Psychology, by JOSIAH ROYCE. The Macmillan Co., New York, 1903. pp. 392.

This handbook is a striking illustration of its author's versatility. Despite his versatility the author has by no means escaped his philosophical bias. This is seen in the treatment of a number of the topics, more so perhaps in his style, and most of all in the topics he has omitted. Further notice will probably follow.

Contemporary Psychology, by GUIDO VILLA. Swan Sonnenschein and Co., London. The Macmillan Co., New York, 1903. pp. 392. Price, \$2.75.

Psychologies nowadays are written from many standpoints and all conceivable inclusions and exclusions. The scope of this work can be seen by the titles of its chapters: history and development of psychology, object and scope, body and mind, methods, psychical functions, composition and development of mental life, consciousness, the law of psychology.

L'Imagination, par L. DUGAS. Octave Doin, Paris, 1903. pp. 340.

This fruitful topic is first discussed in the sphere of sense, will, memory and sentiment; in the second part, creative imagination is treated as spontaneous, reflex, practical, scientific and esthetic. It is especially interesting, giving accounts of theories of the imagination and of images.

The Evolution of Man and His Mind, by S. V. CLEVINGER. Evolution Pub. Co., Chicago, 1903. pp. 615.

The topics treated are as follows: the Aryans, Semites, middle ages, evolution, heredity and degeneracy, superstition, evolution of language and writing, hunger and health, development of mind, evolution of brain, sense and feeling, instincts and emotions, mental diseases, character, sociology, etc. Both the thought and the style of this work are loose. Its range is immense and suggests that the author is writing himself out. Probably he would by no means claim that it makes much original contribution, but it is by no means without interest and value.

Variation in Animals and Plants, by H. M. VERNON. K. Paul Trench, Trübner and Co., London, 1903. pp. 415.

This work discusses measurements of variation, dimorphism, discontinuous and correlated variation, its cause, loss, effects of temperature, light, moisture, salt, food, life conditions generally, and in the third part takes up its relation to evolution. The book is perhaps rather too prominently occupied with the writer's own investigations, but they are interesting and not without value.

Imitation or the Mimetic Force in Nature and Human Nature, by RICHARD STEEL. Simpkin, Marshall, Hamilton, Kent and Co., London, 1900. pp. 197.

The chapters treat of imitation in economics, psychology, ethics, religion, politics, law, custom, fashion, language, poetry, habit, heredity, in the inorganic world, molecular activity, and in reasoning. It treats the subject in a large, general way, without indication of much technical knowledge. Some of the chapters were read before the Literary and Philosophical Society of Liverpool.

Control of Heredity, by CASPER REDFIELD. Monarch Book Co., Chicago, 1903. pp. 343.

This is a very unique work and seems to mark a wholesome transition from the old standpoint of phrenology. The writer discusses inheritance, variation, selection, races of men, eminent families and men, reproduction, longevity, etc. His chief effort seems to be on the basis of a large number of individual cases to maintain the thesis that great men are most likely to be born from post-mature parents.

Essai de Classification Naturelle des Caractères, par CHR. RIBÉRY. F. Alcan, Paris, 1902. pp. 199.

Undaunted by the very limited success of previous efforts in this direction, this author presents a new attempt to classify character, discussing first the difficulties of the problem, the relations between intelligence and character, sensibility and will, emotion and passion. He then classifies temperaments as sensitive-active, amorphous, and tempers, with a final chapter on the education of character.

The Human Race. A sketch of classifications. A chapter in anthropology, by DUREN J. H. WARD. Privately printed. pp. 26.

This little pamphlet is interesting and touches briefly upon many topics. It is the smallest, most condensed, and most systematic epitome of its general topic, perhaps, that now exists.

United States Census Office. Manual of International Classification of Causes of Death. Adopted by the United States Census Office for the Compilation of Mortality Statistics, for Use Beginning with the Year 1900. Prepared under the Supervision of W. A. King. Govt. Print, Washington, 1902. pp. 177.

This elaborate classification of the causes of death is prefaced by an

interesting discussion of the methods of securing uniformity and of the errors in reporting and classifying the causes of death. This certainly suggests a new topic for medical education.

Native Institutions of the Ogowe Tribes of West Central Africa, by R. L. GARNER. Reprinted from the Journal of the African Society, 1902.

This reprint is from the proceedings of a society founded to rescue the memory of vanished races and to work against the extinction of those that yet remain. It is a very interesting and sympathetic study of fetichism and magic.

Les Obsessions et la Psychasthénie, par F. RAYMOND et PIERRE JANET. F. Alcan, Paris, 1903. pp. 543.

This is the second number of the fourth series of publications of the fruitful clinic at La Salpêtrière. It is devoted to neurasthenia, aboulia, defective sentiments, general agitations, algias and phobias, delirium of touch, tics, doubt and folly, obsessions, impulsions, and their treatment, with twenty-two cuts. It is composed on the same plan as a former work of these authors in 1898 entitled *Nevroses et les Idées Fixes*. With the preceding volume, it is believed that data are now supplied for methods of presenting the evolution of the types of these maladies. Janet, in particular, has devoted himself to descriptive psychology in this field, and the two works together make a connected and a more or less complete whole.

Recherches Cliniques et Thérapeutiques sur l'Épilepsie, l'Hystérie et l'Idiotie, par BOURNEVILLE. F. Alcan, Paris, 1902. pp. 236.

The first part of this work is devoted to the history of this service during the year 1901; the second, to medico-pedagogical instruction; and the third, to clinical therapeutics and pathological anatomy.

Le Mensonge, par G. L. DUPRAT. F. Alcan, Paris, 1903. pp. 188.

Lies are first described and classified. Another chapter treats their abnormality. The third is devoted to the lies of childhood. Then follow lies in collective life, in comparative psycho-sociology, their psycho-physiology, lies from the moral and educational point of view respectively.

L'Image Mentale (Évolution et Dissolution), par J. PHILIPPE. F. Alcan, Paris, 1903. pp. 151.

In the first chapter, the mental image is analyzed; the second is devoted to fusion of images; and the third to evolution of the mental image. The work has an experimental basis and is a real contribution to the subject.

Le Dieu de Platon d'après l'Ordre Chronologique des Dialogues, par PIERRE BOVET. H. Kündig, Genève, 1902. pp. 186.

The first part résumés Plato's views on the place of God in his philosophy and in the dialogues which treat of ideas, while the second part is devoted to the God idea of the later dialogues.

Spinoza's Political and Ethical Philosophy, by ROBERT A. DUFF. (James Maclehose and Sons, Glasgow.) The Macmillan Co., New York, 1903. pp. 516. Price, \$3.50.

This book is the first part of a task which has occupied the author for many years and which he hopes to complete, but is a whole by itself. It is solely an elucidation and not a criticism. It seeks to furnish a connected and continuous account of Spinoza's system and to show how his ideas were related to each other. It is a work of great value

to the scholar, shedding light upon many of the most difficult questions connected with this remarkable mind.

Aristote, par CLODIUS PIAT. F. Alcan, Paris, 1903. pp. 396.

The editions, versions, commentaries, monographs and special studies are first described. Then the works, beginning with metaphysics, are briefly discussed under the heads—being, nature, soul and human action. It is a monograph of the Aristotelian system which will serve both the expert and the cultivated general reader.

The Seven Cardinal Virtues, by JAMES STALKER. Hodder and Stoughton, London, 1902. pp. 131.

These are wisdom, courage, temperance, justice, faith, hope, and love, each of which has a chapter. It is on this frame work that the moral system of Aquinas is built. The Seven Deadly Sins was the fit title of a former booklet.

Why the Mind Has a Body, by CHARLES A. STRONG. The Macmillan Co., New York, 1903. pp. 355.

This is a work of love and has absorbed the thought of an earnest, able man for at least a decade. It is divided into three parts—the empirical, which considers the facts and the question of causal relations; metaphysical, which discusses metaphysical principles and their application to problem, with a criticism of theories. We hope to print a fuller review later.

Le Goût, par L. MARCHAND. O. Doin, Paris, 1903. pp. 330.

This work attempts to be a comprehensive account of the sense of taste. It includes a series of original, interesting experiments, and contains thirty-three cuts illustrating the nerves involved, their mechanism, etc.

Morale Essai sur les Principes Théoriques et leur Application aux Circonstances Particulières de la Vie, par HARALD HÖFFDING. Schleicher Frères et Cie, Paris, 1903. pp. 578.

This is a very important work. It discriminates positive and scientific morals from theological and philosophical. The author then proceeds to discuss the methods of ethics, the theory of conscience, liberty and will, the problem of evil and good, of individual and social morality. Under the former, he discusses the affirmation of self, suicide, independence, development and love of truth. Under social morality, he discusses the family, various forms of marriage, culture, its materials as ideals and types, the relations between the church and the State, and philanthropy.

David Hume and His Influence on Philosophy and Theology, by JAMES ORR. Charles Scribner's Sons, New York, 1903. pp. 246. Price, \$1.25.

A clean, attractive book describing Hume's life, his literary labors, relations to previous philosophy, and the chief problems which he discussed, viz.: the first principle of knowledge, cause and effect, free will, substance, the material world, the ego, utilitarianism, miracles, political economy and miscellanies, with an account of the different editions of his work.

De l'Étude des Phénomènes au Point de Vue de leur Problème Particulier, par GASTON GAILLARD. Schleicher Frères et Cie, Paris, 1903. pp. 245.

The issues of philosophical research into particularities, development of algorithms, anti-moralism, special problems and their groups, are treated in this work.

An Introduction to Systematic Philosophy, by WALTER T. MARVIN.
The Macmillan Co., New York, 1903. pp. 572.

This is the most comprehensive book known to the present reviewer. It discusses first the philosophy of nature, then that of mind; ontology, cosmology and cosmogony follow. Part two treats the nature of knowledge, its validity; the world as presupposed by knowledge and its manifold interpretation. Part three treats of the philosophy of religion, theoretical ethics, æsthetics and philosophy as a science.

The Influence of Emerson, by EDWIN D. MEAD. American Unitarian Association, Boston, 1903. pp. 304.

This work, dedicated to Edward Everett Hale, has three parts: (1) the philosophy of Emerson, (2) Emerson and Theodore Parker, (3) Emerson and Carlyle. No better man could be chosen to write such a book than Mr. Mead, and it goes without saying that his work is not only timely but one which all lovers of Emerson will wish to read.

Archiv für die Gesamte Psychologie, von PROF. A. KIRSCHMANN in Toronto (Canada), PROF. E. KRAEPELIN in Heidelberg, PROF. O. KUELPE in Würzburg, DR. A. LEHMANN in Kopenhagen, PROF. G. MARTIUS in Kiel, PROF. G. STÖRRING in Zürich, DR. W. WIRTH in Leipzig and PROF. W. WUNDT in Leipzig. Wilhelm Engelmann, Leipzig, 1903. Vol. I, Parts 1-3.

This new Archiv contains the following foreign papers: measurement of fatigue by Kraepelin; the influence of accessory stimulation on perception of space by Pearce; the possibility of a quantity of tone sensation by Gätschenberger; a report of progress in language of children during the four years ending in 1902 by Gutzmann, inner nutrition and organic sensations by Lipps; difference of tone and consonance by Krüger; on the single and collective achievements of school children by Mayer.

Von der Nervenzelle und der Zelle im Allgemeinen, von PAUL KRONTHAL. Gustav Fischer, Jena, 1902. pp. 274.

This work is divided into two parts, one on the biology of the nerve cell, and the other on cell in general and the nerve cell in particular. At the end are eleven full page reproductions of microscopic specimens in color.

The University of Colorado Studies, edited by Arthur Allin and Francis Ramaley. University of Colo., 1903. Boulder, Vol. I, No. 3. pp. 262.

Dr. Allin has two interesting papers, one on the basis of sociality and the other on the law of future specific and social efficiency.

Studien zu Methodenlehre und Erkenntnisskritik, von FREDERICK DREYER. Wilhelm Engelmann, Leipzig, 1903. pp. 498.

This volume discusses the method of continuity, meta-geometry, possibility, natural and associative complexion, actuality, etc.

Das Ethische Element in der Ästhetik Fichte's und Schelling's, von BENJAMIN F. BATTIN. Kampfe, Jena, 1901.

This thesis discusses chiefly Kant, Fichte and Schelling.

Neurasthenie und Hysterie bei Kindern, von DR. ALFRED SAENGER. S. Karger, Berlin, 1902. pp. 32.

Anthropological Instruction in Iowa, by DUREN J. H. WARD. Reprinted from the July, 1903, number of the Iowa Journal of History and politics, published at Iowa City by the State Historical Society of Iowa. pp. 29.

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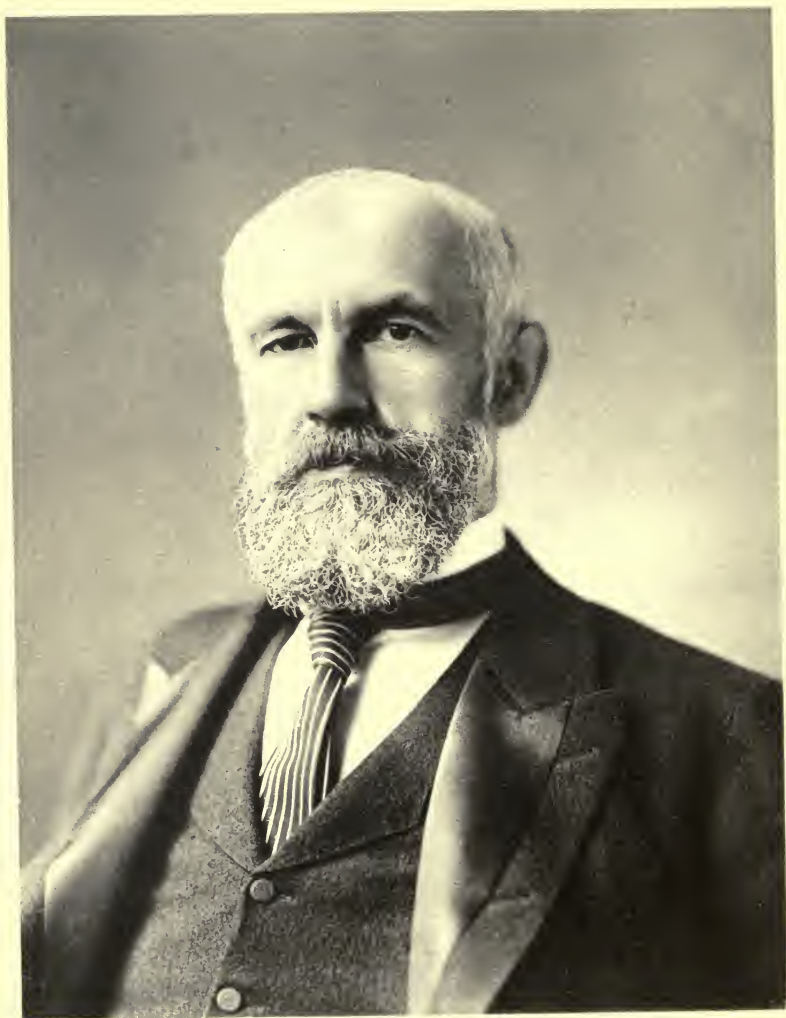
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C. Henry Hall

To

Granville Stanley Hall

Founder of the First American Laboratory for Experimental Psychology and of the first American Journal for the Publication of the Results of Psychological Investigation

Pioneer in the Systematic Study of the Mental Development of Children and in the Application of its Results to Educational Practice

Ardent Inspirer in Others of the Zeal for New Knowledge

in

Commemoration of the Twenty-fifth Anniversary of his Attainment of the Doctorate in Philosophy

This Collection of Papers is Dedicated Conjointly by Colleagues and Former Pupils

TABLE OF CONTENTS.

	PAGE
Contribution à la Psychologie du Rêve, par Professeur H. BEAUNIS,	7
Deception and Reality, by Professor AUGUST KIRSCHMANN,	24
Binocular Vision and the Problem of Knowledge, by Professor JAMES H. HYSLOP,	42
A Critique of 'Fusion,' by Professor I. MADISON BENTLEY,	60
The Genetic Function of Movement and Organic Sensations for Social Consciousness, by Professor MARGARET FLOY WASHBURN,	73
The Status of the Subconscious, by Professor JOSEPH JASTROW,	79
An Attempt at Analysis of the Neurotic Constitution, by ADOLPH MEYER, M. D.,	90
The Psychology of Football, by Professor G. T. W. PATRICK,	104
Retroactive Amnesia: Illustrative Cases and a Tentative Explanation, by Professor W. H. BURNHAM,	118
The State of Death: An Instance of Internal Adaptation, by Professor JAMES H. LEUBA,	133
Primitive Taste-words, by Professor ALEXANDER F. CHAMBERLAIN,	146
On Time Judgments, by BEATRICE EDGELL, Ph. D. Communicated by Professor AUGUSTUS WALLER,	154
Class Experiments and Demonstration Apparatus, by Professor E. B. TITCHENER,	175
Experimental Studies on the Psychology of Music, by Professor MAX MEYER,	192
Ein Beitrag zur Experimentelle Æsthetik, von Professor O. KUELPE,	215
A Study of the Accuracy of the Present Methods of Testing Fatigue, by Professor A. CASWELL, ELLIS and MAUD MARGARET SHIPE,	232
A New Type of Ergograph, with a Discussion of Ergographic Experimentation, by Professor JOHN A. BERGSTRÖM,	246
Attention Waves as a Means of Measuring Fatigue, by Professor W. B. PILLSBURY,	277
Studies in Pitch Discrimination, by GUY MONTROSE WHIPPLE, Ph. D.,	289
Statistics of American Psychologists, by Professor J. MCKEEN CATTELL,	310

	PAGE
A Study on the Conductivity of the Nervous System, by Professor YUJIRO MOTORA,	329
The Relation of Motor Power to Intelligence, by Professor T. L. BOLTON,	351
Are Chromæsthesias Variable? A study of an Individual Case, by Professor F. B. DRESSLAR,	368
On the Guessing of Numbers, by Professor E. C. SANFORD,	383
A Quarter Century of Psychology in America: 1878-1903, by Professor EDWARD FRANKLIN BUCHNER,	402
A Bibliography of the Published Writings of President G. Stanley Hall, by LOUIS N. WILSON,	417

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CONTRIBUTION À LA PSYCHOLOGIE DU RÊVE.

Par H. BEAUNIS,

Directeur honoraire du Laboratoire de Psychologie physiologique
de la Sorbonne.

I.—Je voudrais, dans les pages suivantes, étudier, d'après mes observations personnelles, quelques points de la psychologie du rêve.

Je préciserai d'abord les conditions dans lesquelles ces observations ont été recueillies.

J'ai toujours eu la précaution d'écrire mon rêve immédiatement après mon réveil, sans attendre un seul instant; sans cette précaution, le souvenir du rêve se perdait presque instantanément. Dans quelques cas cependant ce souvenir a pu reparaitre nettement dans la conscience sous une cause occasionnelle, mais ces cas sont exceptionnels.

Avant d'aller plus loin, je dois répondre à quelques objections qui ne tendraient à rien moins qu'à infirmer la valeur du souvenir dans le rêve.

Egger a fait remarquer que, dans les cas d'*oubli partiel*, on est exposé à compléter par l'imagination les fragments incohérents et disjoints fournis par la mémoire. Il est difficile en effet, comme le dit Guardia, de séparer l'observation pure et simple des réflexions rétrospectives. Je suis convaincu pourtant que cette séparation est possible avec un peu d'attention. Il n'y a qu'à éliminer les cas douteux.

D'après Dugas, le sommeil et la veille se confondraient si

bien dans la conscience qu'on se trompe sur le moment précis du réveil; on se croit déjà réveillé quand on est encore endormi. Cette objection est réfutée par ce fait que j'ai plusieurs fois constaté : quand on est réveillé subitement par une personne étrangère, le souvenir du rêve qu'on était en train de faire subsiste et il ne peut exister dans ce cas le moindre doute sur la réalité du réveil.

Goblot a été jusqu'à dire que les seuls rêves dont on se souvient sont ceux qui se rapportent à la période de transition entre le sommeil et le réveil. En ce qui me concerne, cette assertion est aussi inexacte que la précédente. Dans cet état de transition, voici ce que j'observe sur moi-même. L'intelligence est à peine éveillée; les impressions extérieures arrivent affaiblies et un peu vagues; c'est une sorte de torpeur demi-consciente, très agréable du reste, pendant laquelle peuvent apparaître des images, des tableaux assez semblables à ceux du rêve; mais cet état se distingue facilement du sommeil en ce qu'on peut y mettre fin dès qu'on le veut; il suffit pour cela d'ouvrir les yeux; le réveil a lieu immédiatement et, en étudiant ce qui se passe, on se convainc facilement que ces images ne peuvent être confondues avec le rêve.

Dans cette étude je me contenterai de citer quelques exemples en restreignant le plus possible leur nombre et ne donnant que les détails nécessaires.

II.—Par l'analyse, les phénomènes du rêve peuvent se décomposer en trois phases qui se succèdent rapidement.

1°. Une impression provenant soit de l'extérieur (impression sensorielle, bruit, sensation tactile, etc.), soit des organes (sensations internes, sensation musculaires, génitales, etc.), constitue le point de départ initial du rêve, *phase d'excitation initiale*.

2°. L'excitation initiale se transmet à certains centres cérébraux dont elle met en jeu l'activité sous forme de souvenir, *phase du souvenir*.

3°. De ces centres l'excitation s'irradie dans un plus ou moins grand nombre de centres cérébraux sensitifs, moteurs psychiques, et produit ainsi la multiplicité et la variété phénoménales du rêve, *phase d'irradiation*.

En ce qui concerne la première phase, il est certain que

toutes les impressions sensibles provenant soit de l'extérieur, soit des organes peuvent devenir le point de départ de rêves. Chez moi ce point de départ se trouve spécialement dans les sensations tactiles, musculaires et dans les impressions partant des organes digestifs.

On peut se demander si, dans la production du rêve, cette première phase est nécessaire et si les centres cérébraux dans lesquels surgissent les souvenirs ne peuvent pas entrer en activité par de simples variations de pression sanguine, des changements de composition du sang, des actions chimiques, etc., en dehors de toute incitation sensitive. La question est presque impossible à résoudre; cependant je serais porté à répondre par l'affirmative. Certains aliments, le gibier par exemple, qui produisent des toxines, déterminent constamment chez moi des rêves plus fréquents, plus intenses et plus compliqués.

Les souvenirs qui constituent la deuxième phase peuvent se rattacher aux événements soit du jour même, de la veille ou des jours précédents, soit, moins souvent chez moi, de périodes antérieures quelque fois très éloignées. Je dois remarquer que, même dans ce dernier cas, ma personnalité *actuelle* était toujours conservée; jamais je n'ai rêvé que j'étais enfant ou jeune homme, même quand mes rêves se rapportaient à des événements de ces périodes de mon existence.

III. Quoique nous soyons dans l'ignorance la plus absolue des lois qui président à l'enchaînement de ces trois phases et à l'activité psychique des centres cérébraux dans le rêve, il n'en est pas moins intéressant d'étudier la façon dont les divers souvenirs se combinent et s'amalgament pour constituer le rêve. Bien que cette étude ait déjà été faite par tous les auteurs qui se sont occupés de cette question, j'en donnerai quelques exemples.

Rêve A.—J'étais avec G. dans une pièce où se trouvaient deux bibliothèques vitrées. A un moment G. me dit "je vais vous montrer une pièce très curieuse." Il leva le couvercle d'une boîte qui était à terre et je vis une tête barbue, en cire colorée et très bien faite. En regardant du côté opposé, la figure changeait; c'était une vulve entr'ouverte dont les deux lèvres étaient écartées par de petits personnages en cire, nus,

très bien exécutés; à la partie supérieure de la vulve, à la place du clitoris, était un gland assez volumineux.

Le jour même, j'avais vu à Nice, sur une promenade, une fillette de trois ans environ, debout sur un banc, relevant ses jupes au-dessus de la ceinture et montrant ses parties à un petit gamin de quatre ou cinq ans couché sur le banc en face d'elle. D'autre part je m'amuse quelque fois à modeler en cire; mais cela ne m'était pas arrivé depuis longtemps.

Rêve B.—Je me trouvais chez Monsieur P. à Saint Jean de Villefranche, dans une immense salle; en face de moi était un groupe de quelques personnes (trois ou quatre) parmi lesquelles une seule se présentait nettement à ma vue, le peintre M. . . Il était grîmé singulièrement, avait le nez barbouillé de peinture et jouait du violon. Je m'efforçais de l'écouter, mais j'avais sommeil et avais toutes les peines du monde à lutter contre l'envie de dormir. Pour la combattre, je me levai et allai vers lui pour le féliciter sur son jeu. En m'approchant de lui je contemplai à travers une glace sans tain un paysage merveilleux, la mer, les montagnes éclairées par une lumière douce, ravissante. Sur la mer se voyaient des personnages qui semblaient marcher sur l'eau. J'en fis même tout haut la réflexion en rêve. Ma voix me réveilla probablement, car mon rêve se termina là et je me réveillai. Il était cinq heures du matin.

Il me fut très facile de reconstituer la genèse du rêve. Monsieur et Madame P. avaient à Saint-Jean une campagne où ils pratiquaient la plus large hospitalité et où tous les dimanches, il y avait réception ouverte. On y rencontrait tout ce qui portait un nom dans la littérature, les arts, la science, la politique, et le monde officiel y coudoyait la colonie étrangère; c'était une curieuse réunion des personnalités et des types les plus divers. J'y avais entendu quelques jours auparavant une jeune violoniste, Mademoiselle B. et une pièce d'André Theuriet dans laquelle le peintre M. jouait le rôle d'un paysan. La veille même j'avais reçu une invitation à déjeuner de Madame P. et j'avais lu dans un journal deux entre-filets concernant, l'un, le peintre M., l'autre Mademoiselle B.

En outre je m'occupe un peu de photographie. J'avais pris par erreur sur la même plaque une vue de mer et de bateaux

et un groupe de pêcheurs. Il m'avait paru amusant de faire tirer une épreuve de ce cliché et la veille même j'avais été la chercher à Nice chez le photographe. En revenant en tramway de Nice à Villefranche j'avais admiré à travers les glaces du tramway la mer et les montagnes et l'effet de lumière produit par le soleil couchant tamisé par les nuages. L'amalgame de tous ces souvenirs avait constitué mon rêve.

Rêve C.—J'ai rêvé que j'allais faire de l'escrime. Je descendis dans une sorte de sous-sol; j'y trouvai un homme nu jusqu'à la ceinture, Hercule. Du moins je n'avais pas le moindre doute; pour moi c'était Hercule. Je lui palpais les pectoraux, les biceps; il ne voulait pas s'aligner avec moi, me prenant en pitié; mais je lui disais: "en tout cas cela me posera de m'être mesuré avec vous."

Je n'ai jamais fait d'escrime dans ma vie, sauf pendant trois mois en 1857. (Mon rêve est du 19 Juin, 1894.) Mais j'avais lu dans un journal que les Ministres de la guerre et de l'instruction publique avaient interdit aux maîtres d'armes des régiments et des lycées de relever le défi des maîtres d'armes italiens. Je n'ai pu retrouver dans ma mémoire ce qui a pu donner lieu à l'idée d'Hercule; mais je suis certain que dans mon rêve c'était bien l'Hercule mythologique qui était devant moi et non un homme fort ou un hercule quelconque.

Dans certains cas le rêve peut avoir pour point de départ une impression dont on n'a aucun souvenir et qu'on a vue cependant. J'en citerai l'exemple suivant intéressant à plus d'un point de vue.

Rêve D.—Je me trouvais avec un homme assez grand ressemblant à un de mes neveux et une petite fille. Je faisais avec eux des expériences sur la mémoire. Je leur donnais des mots à retenir, quatre, puis cinq. Je m'aperçus tout-à-coup que ce procédé était mauvais. Alors j'employai des chiffres. Je commençai par trois chiffres que je leur lisais tout haut en les inscrivant. L'homme put répéter trois chiffres, la fillette aussi. A quatre chiffres, l'homme fit une erreur, la fillette les répéta tous. A cinq chiffres, l'homme dit qu'il ne pouvait les répéter, la fillette les répéta exactement. Sur ces entrefaites je me réveillai.

L'homme et la fillette se trouvaient à six ou sept mètres de

moi quand je leur lisais les chiffres. Les chiffres que je lisais présentaient ce caractère d'être comme entourés d'une sorte de figure assez compliquée ressemblant vaguement à une forme humaine.

J'avais corrigé des épreuves d'anatomie toute la journée et dans la soirée. J'avais seulement interrompu cette correction pour parcourir des yeux très rapidement un article de la Revue philosophique sur *l'Audition colorée* (Mai, 1893) dans lequel l'auteur, V. Henri, citait le cas d'une personne qui associait à l'idée de chiffre celle d'une forme humaine et je remarquai que j'avais déjà observé un cas semblable. En relisant cet article le matin après avoir transcrit mon rêve, j'y trouvai deux lignes sur la mémoire des chiffres, lignes que je ne me souvins pas du tout avoir lues la veille. J'avais lu des yeux seulement, très vite et n'avais fait attention qu'à l'audition colorée qui seule m'intéressait pour le moment.

Ce rêve présente de l'intérêt à plusieurs points de vue. Je me contenterai de faire remarquer qu'il est difficile de le concilier avec la théorie dans laquelle le rêve ne serait qu'une succession de tableaux.

IV. Avant d'aborder les caractères de mes rêves je noterai les points suivants.

Je n'ai jamais pu rêver à volonté ni déterminer d'avance les sujets et les caractères de mes rêves. Jamais non plus je n'ai pu volontairement mettre fin à mon rêve en me réveillant.

D'après ce que j'ai observé sur moi-même, je suis porté à croire que je rêve toutes les nuits. Je n'irai pas cependant jusqu'à dire qu'il n'y a pas de sommeil sans rêves; ce serait peut-être trop absolu. Mais je crois que c'est seulement dans le *sommeil naturel profond* et dans le *sommeil hypnotique sans suggestions* qu'on rencontre l'absence complète de rêves et l'inactivité absolue de la pensée. (Voir mon livre, *Le somnambulisme provoqué*, 2e Édit. p. 210.) En tout cas les rêves dont je me souviens nettement sont ceux qui se présentent dans la seconde moitié de la nuit.

J'étudierai successivement la façon dont se comportent chez moi les diverses images mentales dans le rêve.

Images visuelles. Je suis un visuel, mais un visuel incomplet. Si je ferme les yeux et que je veuille me représenter

mentalement un objet, un arbre, une personne, l'image mentale n'a jamais la netteté de l'image réelle; les images sont toujours un peu indécises, un peu floues, comme en grisaille; les couleurs sont un peu passées; c'est plutôt une sorte de vision mentale plus facile à comprendre qu'à expliquer. Si j'entends prononcer un mot, je vois le mot imprimé, en caractères ordinaires, plutôt que l'objet lui-même et souvent c'est le mot seul que je vois. Je dois dire que j'ai beaucoup lu et que, soit habitude, soit organization innée, je lis des yeux seulement et très rapidement.

Dans mes rêves, les images visuelles ont à peu près les mêmes caractères de vague et d'indécision; en général les objets sont vus plus nettement que les personnages, les mots plus que les objets, les dessins (dessins d'anatomie et d'histologie par exemple), les cartes plus nettement encore.

Cependant, dans mes rêves *en grisaille* comme je les appelle, certains objets, certaines figures peuvent présenter une netteté de contours et de couleurs presque comparables à la réalité, mais c'est toujours limité à une fraction de l'étendue visuelle du rêve, fraction qui joue le rôle principal et sur laquelle je fixe mon attention. J'ai même conscience que l'effort d'attention augmente la netteté de l'image. En tout cas il est très rare chez moi que les images mentales visuelles soient aussi vives que dans la réalité.

Les *images auditives* ne sont jamais chez moi très intenses; j'entends nettement les voix, mais elles sont comme assourdies, voilées; ma propre voix, sauf de rares exceptions, m'arrive avec les mêmes caractères. Je noterai que, quoiqu'aimant beaucoup la musique et en entendant assez souvent, je n'ai jamais entendu en rêve de symphonies ou d'auditions musicales réelles; tout s'est borné à quelques fragments de café-concert ou à quelques refrains grotesques (une seule fois cependant un air de la *Favorite*). Rien de spécial pour les *sensations tactiles* et les *sensations de température*. Je n'ai ressenti que très rarement de *douleur*, quelque fois seulement un peu de gêne ou de fatigue.

Autant qu'il m'en souviennne, je n'ai jamais eu de sensations *olfactives*. Une seule fois j'ai éprouvé une sensation *gustative* très nette dans le rêve suivant.

Rêve D.—J'étais dans un hôtel. Ma chambre était remplie d'abeilles et, en la quittant, je m'aperçus qu'il y avait à la porte de ma chambre un trou assez large par lequel les abeilles se répandaient dans le corridor dont elles couvraient le tapis. Je me hâtai de descendre pour prévenir du fait le gérant de l'hôtel. En descendant l'escalier, je pris le pan de mon habit et le suçai; il était couvert de miel et avait un goût sucré très prononcé.

En revenant à la campagne la veille, j'avais trouvé ma chambre remplie d'abeilles. Six semaines auparavant un essaim avait pénétré dans la cheminée où il s'était installé; une partie des abeilles était entrée dans ma chambre en passant sous la trappe de la cheminée et le miel avait coulé jusque sur le plancher.

Je ne m'arrêterai pas sur les sensations *génitales*; elles ne présentent rien de particulier.

Les *sensations organiques* jouent chez moi un rôle assez important et constituent les éléments d'un grand nombre de mes rêves. Spécialement les besoins correspondant aux fonctions du rectum et de la vessie se présentent souvent dans mes rêves et parfois dans les circonstances les plus invraisemblables et les plus grotesques sur lesquelles il est inutile d'insister.

Les sensations de mouvement (*images motrices*) sont un des éléments les plus importants de mes rêves. Je ne suis pourtant pas un moteur; quand je lis un mot, je ne le prononce jamais mentalement; cette tendance motrice, cette ébauche de mouvement ne se montre chez moi que pour les chiffres, et très faiblement encore, jamais pour les mots.

Les rêves de mouvement consistent en marches, courses, ascensions quelquefois pénibles, descentes, très rarement chûtes dans des précipices. Un rêve que j'avais souvent autrefois, c'était de voler à un ou deux mètres au-dessus du sol en parcourant ainsi d'un bond léger dix à vingt mètres; c'était pour moi une sensation délicieuse de légèreté et de vitesse en même temps qu'une vive satisfaction d'amour-propre d'avoir résolu le premier le problème de la locomotion aérienne et cela sans mécanisme particulier et en vertu d'une organisation supérieure. Ce rêve a tout-à-fait cessé depuis une vingtaine d'années.

Je n'ai presque jamais rêvé que j'écrivais. J'ai passé cepen-

dant une partie de mon temps à écrire. Le même fait a déjà été signalé par Guardia qui a énormément écrit et n'a jamais rêvé qu'il écrivait. Je n'ai jamais non plus rêvé que je dessinais ou que je modelais, ce qui m'arrive assez souvent en réalité.

Pour ce qui concerne la parole, je renverrai aux rêves intellectuels.

V.—*Sujets des Rêves.* Mes rêves portent tantôt sur des objets inanimés, connus ou non (paysages, monuments, etc.), tantôt sur des personnes connues ou imaginaires; ou bien ce sont des scènes dans lesquelles je joue ordinairement un rôle actif et non pas seulement un rôle purement contemplatif.

Les objets ou les personnes connues ne sont jamais tout-à-fait semblables à la réalité; souvent même il n'y a qu'une ressemblance lointaine, ce qui ne m'empêche pas de les dénommer sans hésitation. Pour les personnes imaginaires il m'arrive souvent de leur attribuer un nom, celui d'un personnage célèbre par exemple, sans que je sache pourquoi (Rêves C, T, U).

J'ai vu assez souvent en rêve des personnes mortes de ma famille ou de mes amis sans que je puisse me rendre compte des motifs qui m'ont fait apparaître telle personne plutôt que telle autre. Ainsi je n'ai jamais vu ni mon père, ni ma mère, tandis que j'ai vu plusieurs fois un frère et une sœur.

Dans ces rêves de personnes décédées, je ne puis être d'accord avec de Sanctis qui prétend que l'image d'une personne chère qu'on a perdue ne peut être jamais vue en rêve que longtemps après la mort. J'aurai occasion de revenir plus loin sur ce genre de rêves.

VI.—J'arrive maintenant aux rêves que j'appellerai *rêves intellectuels*. Je commencerai par donner quelques exemples de rêves de ce genre.

Quelques uns se rapportent à la profession médicale.

Rêve E.—Je rêvais que je faisais un accouchement avec un confrère. La figure de l'enfant était très nette, celle de la mère très peu. Je n'avais pas fait d'accouchement depuis 35 ans; mais j'avais parlé de cette question dans la soirée.

Rêve F.—J'avais devant moi un cas chirurgical, un individu blessé à l'épaulé; la cavité glénoïde était percée dans son

milieu comme cela arrive quelquefois pour la cavité cotyloïde, et cela me paraissait un cas unique que j'analysais minutieusement. Dix jours auparavant j'avais causé avec le Dr. P. d'une déformation de l'épaule qu'il avait constatée chez les porteurs de sable qui déchargent les bateaux.

Les cas suivants se rapportent plus ou moins directement aux fonctions de professeur.

Pendant un certain temps de ma vie je rêvais très fréquemment que je faisais mon cours. C'était toujours le début que je prononçais dans mon rêve et cela représentait environ huit à dix lignes de texte; ma parole était lente, posée, presque hésitante, tandis qu'ordinairement je parle très vite et ai même une tendance à *m'emballer*, tendance contre laquelle je cherchais toujours à réagir. Je dois dire cependant que le début du cours était dit en général assez posément. Ces rêves ont presque complètement disparu depuis que je suis à la retraite et que j'ai cessé mes cours. J'en ai eu pourtant encore un de ce genre pendant que je travaillais à cet article.

Un fait à noter c'est que ces rêves se combinaient fréquemment avec des rêves grotesques; je faisais mon cours en chemise, en caleçon, ce dont j'étais naturellement un peu honteux.

Rêve G.—Je rêvais que je faisais passer un examen de sages-femmes. Je n'en avais pas fait passer depuis dix ans.

Rêve H.—Je rêvais que je disséquais sur une place publique la face postérieure du bulbe et de la protubérance. Du monde allait et venait sur la place.

Rêve I.—Je me trouvais avec R. et P. dans une sorte de salle de conférences. P. parlait de l'histologie du système nerveux central et décrivait les cellules connectives du cerveau. J'exposais à R. que ce n'était pas mon opinion et que ces prétendues cellules connectives étaient en réalité des éléments nerveux.

Rêve J.—J'étais dans une grande salle et j'indiquais à des jeunes filles des poses, des attitudes, des pas. Une de ces jeunes filles avait une robe bleu pâle et je la voyais d'une façon très nette; les figures des autres étaient beaucoup plus vagues. Elles n'exécutaient pas bien ce que je leur disais et je leur répétais toujours: "Comprenez-moi bien, comprenez-moi bien; il ne s'agit pas de danse, mais d'attitudes, de mimique," et je

faisais de grands efforts pour leur faire comprendre ce que je voulais. Ce rêve est du 27 Décembre 1895 (Comparer au rêve M).

Les rêves suivants ont un caractère intellectuel plus prononcé.

Les premiers se rapportent à des préoccupations littéraires.

Rêve K.—Je me trouvais dans une sorte de hangar lisant devant plusieurs personnes, parmi lesquelles Zola, une pièce de théâtre dont je ne pus au réveil me rappeler le sujet.

Rêve L.—Quelques jours après le rêve précédent je rêvais que j'allais présenter à un concours littéraire une pièce de théâtre ayant pour titre: *les Ambitieux*. Un de mes amis, romancier connu, présentait une pièce au même concours.

Rêve M.—J'avais fait un scenario de pantomime. Je me trouvais dans la rue avec un grand nombre de personnes qui parlaient de ce scenario. A mon réveil impossible de m'en rappeler le sujet. Je dois dire qu'il m'arrive parfois de m'amuser à chercher des sujets de pièces. Ce rêve date du 5 Juillet 1894. (Comparer au rêve J).

Rêve N.—J'avais fait une pièce de théâtre intitulée, *Drame de famille*. L'actrice qui l'avait jouée était avec moi et j'apprenais par elle que ma pièce avait été jouée.

D'autres rêves se rapportent à la musique.

Rêve O.—J'expliquais devant un auditoire la formation de la gamme. Je m'étais beaucoup occupé de cette question les jours précédents.

Rêve P.—Ph. devait passer prochainement une thèse de doctorat sur le *mi*. Je discutais avec lui sur le rôle du *mi* dans la gamme. C'était une dissertation à perte de vue et il me semblait que nous arrivions à des aperçus nouveaux sur le rôle de la médiate.

Je me contente de ces quelques exemples dont j'abrège les détails, voulant surtout montrer ici les différentes directions dans lesquelles peut s'exercer chez moi l'activité intellectuelle dans le rêve. Je passe maintenant à quelques exemples de rêves dans lesquels les manifestations psychiques ont un caractère plus élevé et dans lesquels l'automatisme cérébral est moins puissant.

Rêve Q.—Je racontais à J. avec toutes les explications une

illusion de l'orientation que j'avais observée dans le voyage de Paris à Nancy.

Rêve R.—Je me trouvais en présence de mon ami S. mort depuis peu de temps; il était couché dans son lit comme je l'avais vu pendant sa maladie. Très étonné de le voir vivant je lui demandai comment cela se faisait. Son frère qui se trouvait à côté de lui me dit: "Je vous expliquerai cela." S. me parla, mais je ne me rappelai pas ses paroles à mon réveil. Ce que je me rappelai très bien par exemple, c'est que je réfléchissais comment cela pouvait se faire. Cependant la chose ne me surprenait pas outre mesure; mais je faisais le raisonnement suivant dont je me souvins très bien au réveil: ou je rêve et je le saurai quand je me réveillerai; ou cela est vrai, il est vivant et comment cela peut-il se faire? La mort de mon pauvre ami S. m'avait très vivement impressionné.

Rêve S.—Ce rêve a déjà été publié par moi dans mon livre sur *le somnambulisme provoqué* (2e Edit. p. 299). Je l'abrège. Je voyais un paysage avec de l'eau, de la verdure, des arbres, des maisons, et tous les objets m'apparaissaient dans leurs contours et leurs couleurs avec toute leur netteté. Ce que je me rappelai parfaitement au réveil, c'est qu'en considérant ce paysage je pensai immédiatement à la question qui m'avait souvent occupé de la netteté des images dans le rêve et j'ajoutai même mentalement: cette fois, je suis bien sûr d'avoir vu ce paysage comme si je l'avais réellement devant les yeux. *J'avais très distinctement la conscience que je rêvais et que je raisonnais sur la netteté de mon rêve*, et que je conclusais à l'identité d'aspect des images rêvées et des images réelles. J'ai eu dans d'autres rêves la conscience vague que je rêvais; mais c'est seulement dans les deux rêves précédents que cette conscience s'est formulée d'une façon absolument nette.

Rêve T.—Je me trouvais dans la même salle que Séverine, une autre femme et un homme qui écrivaient à une table. Ils décrivaient leur état d'âme, une sorte d'analyse psychologique de leur moi et me demandaient des éclaircissements. J'avais lu dans la journée un premier-Paris de Séverine et un article de journal sur la dualité du cerveau.

Rêve U.—Je me trouvais avec Maurice Barrès dans un jardin. Nous avions un entretien psychologique sur le rôle de

cet auteur en littérature et sur sa psychologie. Je me souvenais très nettement au réveil que Maurice Barrès s'attribuait la qualification de "*locatif*," tandis que je soutenais qu'il fallait lui attribuer celle de "*personnel*" et nous avions une discussion sur la valeur métaphysique des termes "*locatif*" et "*personnel*." J'avais lu le jour même trois articles de Maurice Barrès. Ce rêve eut lieu à la date du 22 Avril 1894.

On voit par ces exemples que l'activité psychique peut dans certains cas se manifester dans le rêve dans des limites assez étendues et qu'elle peut se traduire par le fonctionnement des centres cérébraux supérieurs. On peut en effet dans le rêve analyser, comparer, juger, raisonner; l'attention peut se porter volontairement sur tel ou tel objet; on discute les questions les plus abstraites; la plupart du temps les raisonnements sont faux, les discussions étranges, les conclusions erronées, mais il n'en est pas toujours ainsi.

En tout cas je puis conclure de mes observations que la conscience subsiste dans le rêve, dans son intégrité et je ne pourrais admettre la distinction qui me paraît tout-à-fait arbitraire que font Spitta et Radestock entre la conscience de soi qui serait supprimée dans le rêve et la simple conscience qui serait seule conservée. La personnalité et le sentiment du moi ont toujours été conservées dans mes rêves. Je n'ai jamais observé le dédoublement du moi dont ont parlé quelques auteurs.

VII.—Les *sentiments affectifs* sont en général très affaiblis chez moi dans le rêve et bien moins intenses que dans la réalité. Jamais de terreurs, sauf dans les cauchemars que j'avais étant enfant, jamais de peurs réelles; un peu d'appréhension (rêves dans lesquels je courais quelque danger, rêve que j'allais être opéré, etc.,) un peu d'inquiétude, de honte (certains rêves où je jouais un rôle ridicule), de l'ennui, c'est à cela que se bornent en général les émotions désagréables de mes rêves. Les *sentiments agréables* sont plus intenses (plaisir de la locomotion aérienne, ravissement devant un beau paysage). Le sentiment de l'amour-propre si j'en juge d'après quelques cas est assez prononcé (Rêves C., F., P.; rêves de locomotion aérienne).

Un fait à noter, c'est que dans les rêves de personnes mortes, je n'éprouve ni douleur de les avoir perdues, ni joie de les

revoir vivantes en rêve. Je n'éprouve d'autre sentiment que de l'étonnement, de la surprise (Rêve R.). Il en est de même quand je rêve que des personnes vivantes sont mortes, même des personnes qui me sont chères.

VIII.—Dans le cours de mon existence déjà longue (je suis né en 1830), j'ai pu suivre l'évolution de mes rêves et les variations que l'âge a pu leur faire subir. Laisant de côté mon enfance et mon adolescence sur lesquelles mes souvenirs ne sont pas assez précis, je puis dire d'une façon générale que les sujets de mes rêves ont varié suivant mes occupations habituelles.

Jusqu'à 30 à 35 ans mes rêves étaient surtout des rêves visuels (paysages, villes, etc.,) et des rêves de mouvement, parmi lesquels je mentionnerai ce rêve de vol aérien qui se répétait fréquemment. Dès que mes fonctions de professeur commencèrent, je rêvais très souvent que je faisais mon cours et ce genre de rêve cessa presque complètement quand je pris ma retraite. Vers l'âge de cinquante ans les rêves de locomotion aérienne disparurent tout-à-fait. Les rêves à proprement parler intellectuels ont commencé quand je m'occupai spécialement de questions psychologiques et surtout quand je fus nommé Directeur d'un Laboratoire de Psychologie physiologique. Les rêves littéraires coïncident avec une période où ma santé m'interdisant tout travail scientifique, je me rejetais sur des travaux littéraires auxquels j'avais toujours pris du reste un très vif plaisir. Les rêves à caractère grotesque ont à peu près disparu depuis une dizaine d'années.

A propos de l'influence des préoccupations habituelles sur les sujets des rêves, je mentionnerai les deux exceptions suivantes.

Pendant plusieurs années à Nancy j'ai consacré beaucoup de temps à l'étude des phénomènes de l'hypnotisme, et cette question me préoccupait vivement tant au point de vue physiologique qu'au point de vue psychologique. Jamais pourtant je n'ai rêvé que j'hypnotisais ou fait un rêve se rapportant à cette question.

Pendant la guerre de 1870-1871, j'ai subi le siège de Strasbourg où j'avais un service d'hôpital; j'ai fait dans les conditions que l'on sait et que j'ai décrites (*Impressions de campagne*) les campagnes de la Loire et de l'Est comme médecin

en chef d'ambulance. Jamais, dans cette période où tout mon être n'avait qu'une pensée, la guerre à laquelle j'assistais, jamais je n'en ai rêvé.

Si les sujets de mes rêves ont varié, comme je l'ai dit plus haut, dans le cours de mon existence, les caractères même de mes rêves n'ont pas varié. Les images mentales se sont toujours présentées dans les mêmes conditions; ce sont toujours les mêmes tableaux en grisaille, les mêmes voix assourdies et la description que j'en ai faite ci-dessus reste valable pour toutes les périodes de mon existence.

Actuellement mes rêves sont surtout des rêves visuels dans lesquels l'élément moteur joue un rôle de moins en moins considérable.

IX.—Jusqu'ici les auteurs qui se sont occupés de cette question ont étudié surtout l'influence des phénomènes psychiques de la veille sur le rêve; il me semble que l'autre face de la question, savoir *l'influence du rêve sur les phénomènes psychiques de la veille* mérite aussi d'être étudiée et j'essaierai, aussi brièvement que possible, d'en faire ressortir l'importance. S'il y a une action des idées sur les rêves, il y a aussi une réaction inverse du rêve sur les idées.

On sait quel rôle considérable jouaient les songes chez les peuples primitifs et chez les anciens; même encore aujourd'hui, dans les classes inférieures cette influence s'est conservée et la *clef des songes* constitue avec l'Almanach et les histoires de brigands le fond de la bibliothèque du colporteur. Chez les êtres ignorants et grossiers, comme on le voit dans les temps barbares ou dans les époques troublées, comme au Moyen-âge par exemple, les rêves prenaient une importance dont il nous est difficile actuellement de nous faire une idée. Les légendes qu'on trouve à l'origine de toutes les religions, les croyances aux êtres fantastiques les plus invraisemblables, les visions, des mystiques, les manifestations quelquefois si étranges de l'art primitif (hindou, étrusque, etc.,) ont en grande partie leur point de départ dans les souvenirs du rêve. Du rêve à la vision, il n'y a qu'un pas à franchir. La vision n'est qu'un rêve prolongé qui a laissé son empreinte dans un cerveau surmené, surexcitable et malade. L'Apocalypse de St. Jean n'est qu'un long rêve sur lequel a vécu le Moyen-âge. Ce sont les rêves

des mystiques qui ont engendré cette doctrine de l'Adoration du Sacré-Cœur qu'a transformé le catholicisme, et on sait quelle est aujourd'hui l'influence de cette doctrine sur les consciences.

Si on envisage cette question du rêve à un point de vue plus strictement philosophique, on peut dire, fait déjà entrevu par quelques auteurs, que la croyance à la survivance après la mort a son germe dans le rêve. C'est, du moins me semble-t-il, la seule explication rationnelle qu'on puisse en donner, si l'on consent à lui chercher une explication rationnelle.

Voilà un homme primitif, ignorant, qui dans un rêve voit apparaître un être qu'il a perdu, père, frère, compagne. Cet être lui parle, va, vient, agit, dans les occupations auxquelles il se livrait de son vivant. Il en conclura naturellement que cet être n'est pas mort tout-à-fait et que quelque chose de lui survit après le trépas. Ce qui survit ainsi ne peut être le corps lui-même qui se corrompt et se détruit. C'est donc quelque chose à côté du corps et distinct de lui. Comme les images du rêve sont en général peu intenses et affaiblies, ce quelque chose qui survit doit être une sorte de forme vague, un *double*, une *ombre*. Puis graduellement, cette idée qui ne s'est développée, ni en un jour, ni chez un seul homme, mais par une lente élaboration dans une série de générations, cette idée se transforme et s'épure; de la croyance grossière à une ombre qui survit à la mort avec les mêmes goûts et les mêmes occupations que pendant la vie, se dégage peu à peu, dans ses diverses manifestations, la conception philosophique et religieuse d'une âme immortelle et d'une vie future, avec son cortège de récompenses et de châtiments. Je me borne à ces considérations que je ne puis développer ici. Elles suffisent pour montrer le rôle que joue le rêve dans les phénomènes psychiques et quelle est aussi son importance dans l'évolution religieuse et morale de l'humanité.

X.—*Conclusions.* Des observations et des considérations qui précèdent je tirerai les conclusions suivantes.

1°. En prenant les précautions nécessaires, on peut avoir confiance dans les souvenirs des rêves tels qu'ils se présentent au réveil.

2°. Les phénomènes du rêve peuvent se décomposer en

trois phases: phase d'excitation initiale, phase de souvenir, phase d'irradiation.

3°. La seconde phase semble pouvoir se produire en dehors de toute excitation initiale sensitive, sous une simple variation de pression ou de composition du sang (action chimique) qui agit directement sur un centre cérébral pour déterminer l'apparition d'un souvenir, point de départ du rêve.

4°. Les souvenirs qui apparaissent dans les rêves peuvent provenir d'évènements du jour même ou des jours précédents ou d'époques plus ou moins éloignées. Les deux ordres de souvenirs peuvent s'amalgamer dans le même rêve.

5°. D'une façon générale les sujets des rêves correspondent aux occupations habituelles.

6°. L'évolution biologique du rêve correspond assez exactement à l'évolution organique et psychologique de l'individu.

7°. Les cas sont fréquents dans lesquels le rêve ne peut être ramené à une simple succession de tableaux.

8°. Les sentiments affectifs sont conservés dans le rêve, mais atténués. Cependant chez moi les sentiments de plaisir et d'amour-propre restent très vifs encore.

9°. La personnalité *actuelle* est conservée dans le rêve.

10°. La conscience de soi est conservée dans le rêve.

11°. On peut, dans un rêve, avoir conscience qu'on rêve.

12°. Les manifestations psychiques les plus élevées, raisonnement, attention, comparaison, jugement, etc., peuvent se montrer dans le rêve (rêves intellectuels).

13°. La volonté peut être conservée dans le rêve, mais elle est affaiblie. En ce qui me concerne je n'ai jamais pu me réveiller *volontairement* au milieu d'un rêve.

14°. L'influence des rêves sur les idées et par suite sur les doctrines philosophiques et religieuses a été méconnue et mérite d'être étudiée.

15°. Le rôle du rêve a surtout été très important chez les peuples primitifs et chez les peuples anciens.

16°. Les visions ne sont que des rêves prolongés et transformés.

17°. La croyance à la survivance après la mort et à la vie future avec toutes ses conséquences philosophiques et religieuses a son germe dans le rêve.

DECEPTION AND REALITY.¹

By PROFESSOR AUGUST KIRSCHMANN, University of Toronto.

Mankind is navigating the ship of its destiny, like the vessels of the ocean, at the boundary of two media. There is the limited sea of possible knowledge, whose depth man has sounded at a few points, whilst for the greater part he has not been able yet to reach the bottom. And there is the unlimited ethereal sphere of belief, into which he peers, but into which he can raise himself only to a little height, and that not without danger of an awkward fall. Now the reason for this failure to reach the solid bottom of the ocean—*i. e.*, the certain foundation of complex experience—and to safely soar through the loftiest heights of the atmosphere—*i. e.*, the realm of belief—lies chiefly therein that man insists, and perhaps thinks that he has to insist, according to his nature, in retaining the conditions which prevail at that boundary surface at which he is accustomed to live and move; where his keel and propeller and rudder dip a few feet or rods into the ocean of certainty, whilst his masts with their swelling sails, and the machinery which furnishes the propelling force are all in the atmosphere of belief. When he pretends to dive down into the depth of the ocean, be it with the submarine boat of the physical sciences or with the rather unfashionable diver's suit of the modern Psychologist, he always carries with him, or has pumped down to him, a quantity of that ever life-inspiring air of belief. And when he rises with balloons or flying machines, his airship, no matter whether it imitates more the fish or the bird,

¹Asked to contribute to this commemorative number in honor of Dr. Stanley Hall I thought I could not do better than by choosing a subject, which, though as old as Philosophy, has never ceased to be of fundamental interest to the Philosopher as well as to the Psychologist and Pedagogue. Though perhaps not in literal agreement with his own views, the following considerations are intended to be in the direction of the noble aims of Stanley Hall, the Nestor of the Experimental Psychologists on this continent and the first and foremost one to apply the new science to the practical problems of education.

is absolutely uncontrollable unless he condescends to make constant use of the strong though invisible bands of that force which continually draws him downward toward his surface, viz., gravity, *i. e.*, attraction of the masses.

Thus man is not only materially but also philosophically a surface animal. But instead of admitting this duality of his intellectual nature—which is at once advantageous in giving him a marvellous facility of motion over that limiting surface, and disadvantageous in confining him more or less to it—and trying to improve his implements (as the submarine and aeronautic engineers do) and methods, he prefers to declare his limitations a deception and vainly grasps for something, which he persuades himself is beyond either of these “deceptive” spheres of his limitations. From the oldest times to the present day philosophers have indulged in hunting for a Reality behind the Given. They have said “the world of the senses is a deception, an illusion, behind which stands a real world of entities unknown and imperceptible to us;” and even those who claimed to avoid this error, and who called themselves Empiricists, did not start, as they pretended, with experience as it is given, but took uncritically for experience that which mankind had been accustomed for some thousands of years to call such. Just the Empiricists thought it expedient to degrade the “secondary” qualities as subjective, and correspondingly untrustworthy, although these secondary and subjective qualities are the ones which are immediately given and of undeniable reality. Spiritualists and materialists agreed as to the necessity of assuming an unchanging substance behind the bewildering and never-ceasing flux of “deceiving” phenomena. Realists and Nominalists, Rationalists and Empiricists, Idealists and Materialists, all agreed as to the legitimacy and the fundamental significance of the problem:

WHAT IS THE REAL?

Now we must emphatically deny the legitimacy of this question altogether. A test of solidity can only be made from a solid ground. To ask for a Reality is in itself a vicious circle; for you can only ask for it, if you are standing in it. We must have the reality in order to be able to ask for it or to question

it. Similarly, it is the greatest fallacy to assume, that starting from an uncertainty we could ever reach the certain, and no philosophic doctrine can be more unfounded than that which claims to start with and from doubt. On the contrary, standing amidst a multitude of undeniable, given realities, we claim the first question, the legitimate and necessary problem, which stands at the beginning of every consistent philosophic system should be just the opposite of the above question. We should ask:

IS THERE ANYTHING UNREAL? AND WHAT IS UNREAL?

It has always been claimed, either tacitly or expressly—though most Philosophers have long seen the inconsistency of the view—that our sense impressions do not furnish us a true image of reality. They more or less deceive us. In the ordinary perception of objects this deception is said to be at its lowest degree, in the case of an optical illusion; a mirage, in a higher degree; and finally in a hallucination it reaches its maximum. Let us examine the matter. We shall refer to the case of the mirage first. Is it really our senses which deceive us, when we see such a product of an unusual course of the rays of light? The traveller in the desert sees in the distance what he thinks is an oasis with green trees and a spring of refreshing water. But do his senses really tell that it is such? I think not; they only tell him that such and such parts of his vision field are filled with light sensations of such and such quality, saturation, intensity and space-configuration. And there can be no doubt about the "reality" of that which the senses directly give. But the traveller interprets these directly given facts. He infers: In the fifty or hundred or thousand cases, where I have had similar experiences, there followed, on approaching, such and such other sensations of sight and touch, etc. Since it has been so for so many times, it must be so this time. Here is where the deception comes in; for, no inference drawn from analogy or per inductionem can ever have complete certainty. All he could say, even if he had a million of positive cases before him, is, that he wished and believed it to be so. Thus not his senses deceive the traveller, but his own interpretation of what his senses give him does it; his unwillingness to admit, *that we can have certainty only about mathematical relations and about the*

actual, that it is the present content of consciousness, never about that of the future.

And so it is in all cases where we interpret the actually given states of consciousness with regard to coming events. The mirage and the ordinary perception of objects are only different in degree in this respect. For, if "I see an apple," the perception of sight at the time being does not include that side of the apple, which I do not see at that moment, the interior of the apple, nor its supposed effect on other senses. Whether certain expectations I have in the course of my interpretation of the given perceptions will later be realized or not is not a confirmation of, nor a detriment to, the reality of the facts given. Even a hallucination is not different in this sense. Whether an experience is a hallucination or a so called "real impression" is largely a question of majority, *i. e.*, of relative confirmation by other instances or other observers. A hallucination which everybody or even only the majority of people should experience could not be distinguished from what is called a 'real impression.' A man living alone on an island and not trained to make conclusions by analogy and induction would have absolutely no criterion for the distinction between reality and hallucination.

We call our dreams unreal, not because the directly given facts, which constitute them, *i. e.*, sensations and emotions, are less vivid or less "real" than those we experience in what we call our waking condition, but because their connections among themselves and with those of the waking state are of different character. The law of causality seems to be partially suspended in our dreams. But we must not forget that the law of causality is the result of induction and therefore not a matter of certainty but one of Belief. Thus the problem of the reality or unreality of dreams, too, is not a question of the facts concerned, but of the interpretation of the facts. If somebody who believes that everything in this world must have a purpose would ask me: What are dreams for? I would unhesitatingly answer: *To remind us of the falsehood of our conception of Reality.* I can easily imagine that some reader will be inclined at this point to accuse me of mysticism. But I claim, not the thinkers who hold with regard to certainty a subjectivistic view (claiming that certainty can never go beyond the subject)

but just those ones are the mystics, who try to establish two different kinds of reality, one for the states of consciousness, *i. e.* the given facts, the other for the so called "real objects," though they are unable to explain and define, or even to describe their "real objects" by anything else than by states of consciousness.

Let us now consider the case of an optical "illusion." Say a line which "*is* straight" is seen as curved. The phrase, the line really "*is*" straight, is only an inaccurate way of stating that under certain other conditions (different standpoint of the observer, presence or absence of certain other lines, etc.) we have the impression of straightness. No matter, whether the majority of cases or conditions or something else is responsible for our decision about the "real" nature of the line, we must insist that the line, when perceived as curved is just as real as when seen straight. Nobody has expressed himself more clearly on this point than Professor Wundt, when refuting the theory that optical illusions are based on erroneous judgment. Not the illusion, which, according to him, is an immediate perception, rests on judgment but the proposition that the perception contains an illusion.

This may be further illustrated by reference to contrast phenomena. A gray paper on a green ground assumes a reddish hue. Helmholtz calls such contrast effects illusions, due to false or erroneous judgments. He does so by virtue of the pre-conceived—though almost universally accepted—opinion, that the inferred statements about objects, as for instance "this object *is* red," imply a higher degree of reality than the statements of directly given facts, as *e. g.*, "I see red."

If you throw at day time by means of an electric arc lantern, with objectives removed but provided with a colored glass, say blue-green, a diffuse light—showing no sharp outlines—on a gray or white wall, the color of this secondary illumination will scarcely be noticed, while the shadows on it, *i. e.*, the places where there is only diffuse daylight, will appear in a beautiful rose or pink color. Nobody can fail to notice that in this case the contrast colors are by far more vivid and conspicuous than the "real" ones in operation, and people who know nothing about contrast, and about the instrumental ar-

rangement of this experiment, will swear that there "is" red light. Everybody will see red. Now what is there erroneous about this judgment which states nothing but a fact? Indeed, if you should succeed in persuading an observer, by showing him that there "is" physically only colorless light, to state that it is only an illusion, this will be an erroneous judgment, for he no longer states what he "sees," but what, on account of customary considerations about "real" objects, he believes "is" there. If we look through proper glasses (*e. g.* red and blue-green) at a landscape stereoscopically projected on a screen (left and right picture, each in one of the above colors, superposed, using as transparent screen a large plate of ground glass and disguising the edges of the not entirely overlapping pictures by appropriate drapery), we have the impression as if we looked through an open window at a real landscape. That, nevertheless, we are not convinced of the reality of the objects seen, is not due to inferior or deviating properties of the perceptions of sight in this case, but to the circumstance that we have other senses to control that of sight, namely the dermal and kinaesthetic senses. But the data which these senses give have no reality superior to that of the sense of sight. If we could at the same time, where we produce artificially the stereoscopic effect for the sense of sight, deceive correspondingly the other senses, the "illusion would be perfect;" *i. e.*, we would by no means be able to distinguish whether it was "sham" or "reality."

The definition of a "real" impression in contradistinction to a "deceptive" illusion or hallucination is usually accomplished by reference to the "real object." It is said, a real impression is one caused by a real object. But this is so obviously arguing in a circle, that, I think, a modern Psychologist should be ashamed of such a definition. For, it is quite plain, that what he calls a real object is itself nothing but an impression or a complex of impressions. The distinction "real" or "not real" is—and indeed quite arbitrarily—based on the presence or absence of certain impressions. Consequently we cannot base any classification of the impressions on the reality of the object. The impression is the immediately given and the simple, and the object is the complex and

inferred. Modern Psychology has done marvellous work in tracing the quantitative relations between the physical and the psychical, between stimulus and sensation. But if the modern Psychologist insists upon defining the "stimulus" as something else than a complex of sensations, he is standing still on the standpoint of the so-called naïve realism or better naïve materialism, which is characterized by its superficiality and gross ignorance of the problems.

The great psychological problem should not be: How does it happen that such and such quantities of the physical stimulus only produce such and such quantities of sensation? Properly speaking it should be this: *How does it happen that starting from such and such given facts, viz., the quantities of perceived sensations, we arrive at the idea of such and such stimuli?* It will be objected, that such a view would lead to subjectivism. But first I claim that we have to treat the questions with regard to *truth* and not with regard to *what they lead to*. And, secondly, the above view leads to subjectivism only in so far as it refers to absolute *knowledge*, not with regard to *belief*. And there is a great difference between the solipsistic subjectivist who declares: "There exists nothing but I myself;" and the subjectivist of the above view—if it can be called subjectivism at all—who modestly confesses, "I can have certainty only about that which takes part in my consciousness." My knowledge (*i. e.*, certainty) is extremely limited; but the sphere of belief is unlimited. And he may add: Do not think that I regard belief as something inferior to or less essential in life than knowledge. I only regard the two as *different* and claim we should not confuse them, as we continually do, and indeed not only in ordinary life, but also in science and religion. It must be emphasized that we cannot make a single step even in the most exact science unless we get impulse and direction from belief. But in the propositions, which we formulate as the results of our philosophic and scientific investigations, we should keep carefully separate *elements of knowledge* (absolute certainty) and *elements of belief*. The greatest errors which mankind have made at the expense of their material welfare and intellectual progress can be traced back to the neglect of this simple rule.

Besides all this the words "real" and "reality" are used quite ambiguously. By real is sometimes meant that which has part in consciousness. In this sense, everything is real that involves no contradiction; the idea of freedom, love and hatred, the memory image of a past event and the imagination of the future. All states of consciousness then are real. But there is another, quite arbitrarily chosen meaning of the above terms, according to which the criterion of reality consists in certain special relations in space and time. Thus the impression of a person or object seen at the time is called real, whilst the memory image or imagination of such object or person is denied the character of reality. There is indeed an essential difference, not only with reference to intensity, as some Psychologists have claimed. The images of memory and imagination, though possessing space relations, lack that definite localization in visual space and that palpableness for the sense of touch which characterizes the direct impressions. But that they have not all the properties of the latter does not justify us in treating them as if they had no properties at all. Thus we should not say that memory images and products of imagination are not real, for they are just as real as other states of consciousness. The only thing we could say is, that they do not have certain properties which certain other states of consciousness possess. Thus if we were to speak correctly we should either attribute reality to both classes of presentations and distinguish them according to their characteristic properties; or, if we would agree to designate that definiteness and palpability in space and time as "reality in a narrower sense," we should be careful never to confuse the two meanings. But they are constantly confused by the majority of people, and a good deal of the struggle between philosophical systems arises from this confusion. Most people are not aware of the fact that they refer to different kinds of reality, when they say: "The love of my country is real" and "my imagination of a dragon is not real."

No term is less appropriate as a basis for a classification of philosophical systems than "real," as can be already seen from the fact, that what in the time of the Scholastics was called "Realism" should, according to the present nomencla-

ture, be styled as extreme idealism. The term Realism must, on closer inspection, appear fallacious and misleading, or at least superfluous. If we call him a Realist who assumes that *something* is real, then everybody is a realist. Thus, for instance, the idealist thinks that his conceptions and ideas are the real. And even the most radical sceptic assumes the reality of his doubts; and his whole system commits suicide at the very moment of its birth, if he drops this last reality.

Thus "Realism" cannot designate the theory which believes in *some* reality. There must be added an arbitrary decision excluding certain experiences or facts, or properties of facts, in favor of certain others, from the domain of the real. But then we claim the term is superfluous, for we have in this case a sufficient number of well recognized designations. Thus, *e. g.*, if the realist was said to be he who declares only the above mentioned palpable space properties the true criterion of reality, the term is superfluous, for we have for this view the word materialism. If, on the other hand, Realism is defined as the view which admits of other realities besides the given states of consciousness, we have already the terms Dualism and Phenomenalism, etc.

The term Realism, even if not ambiguously employed, must always be misleading, for it suggests an opposition which it never can have. There is neither a contrary nor a contradictory opposite possible to *the real*. The "Unreal" and the "Nothing" reveal themselves as empty words, pseudoconceptions, as soon as we try to predicate anything of them. I was once heartily laughed at by the students, when I declared, mathematically I could tell them how the Lord created the world. I said it was very simple: He took "nothing" and divided it by "nothing" (%) and then He had everything He wanted. The students who at first glance certainly regarded this as an unbecoming jest, unworthy of the classroom, changed their minds when we discussed the matter, and when they saw, that there is no such thing (perception, presentation, idea or conception) as "Nothing." The fisherman who comes home saying "I caught nothing" only uses inaccurate language which here stands for: I did not catch any fish. And the person who, as a consequence of a posthypnotic suggestion, did not

notice a certain other person and when asked, what he saw in that place, answered "Nothing," simply told a lie. For if you see at all, every part of your vision field must be always filled with some sensation, and even the blind man does not speak correctly, if he says "I see nothing" instead of "I do not see at all." When we speak of "Nothing" we do not mean the absence of all reality; we only mean the absence of something (*i. e.* property or fact, or state of consciousness) replaced by something else.

With regard to the second part of the above mentioned mathematical joke it must not be forgotten that "dividing by nothing" is by no means identical with "not dividing," which is dividing by *one*. "Nothing" and "Dividing by nothing" are terms, with which the naïve mind believes itself to be familiar, and which it consequently uses as if they were backed by some ideas, whilst they are only empty words, not even accompanied by the slightest imagination. Thus, even if we should be inclined to say that "Nothing," the coveted "Nirvana" exists only in the mind of some philosopher, we should say too much; for what exists there is only the word, accompanied by the silent or express but untrue statement, by which the philosopher deceives himself or others, that he has ideas, which correspond to the alleged meaning of that word.

There is only one way to get an opposite to the "Real," namely if we make it identical with the "True." Then the "Untrue," *i. e.*, the product of lying, is "unreal," with which we may well agree, though it must not be forgotten that what is untrue, unreal in the lie is the *meaning* attributed to the actions or words which constitute the lie, not the actions or words themselves, for they are as states of consciousness as real as others. In other words, Lie, the Untruth or the Unreal, no matter whether regarded from the standpoint of the speaker or that of the hearer, *is never a matter of fact, but always a matter of interpretation or statement*. Where there is no interpretation (either a statement in presentations and conceptions to one's self or in words or other symbolic actions to others) there can be no untruth, no unreal.

Thus we come to the important conclusion, *that there can be nothing untrue in this world but men's statements or utterances*

(perhaps animals' too). *If there is anything unreal in this world it must be and can only be the product of human lie.*

And further: Since statement and interpretation are what they are only by virtue of their character as voluntary acts, since no statement or interpretation is possible without the will to state or interpret, *there can be no untruth without a will to lie* (there is no such thing as objective untruth) *and there can be nothing unreal unless there is a will to produce such by lying.*

Here we are at the very pivot on which all the problems of clairvoyance, of prophecy and inspiration hinge. How can there be an untruth in a statement, if there is not the slightest will to be untrue in the speaker and no trace of a will to wrongly interpret in the hearer? If a man speaks without letting his inclinations and desires interfere with truth, if he completely avoids that careless lying which characterizes our ordinary use of language, if there is no trace of an intention in him at the time of deviating from truth, and if the hearer in his interpretation of what he hears is equally free of the will to lie, can there be anything untrue or unreal in what is uttered and heard? There can be nothing false, erroneous or fallacious in it; *it must be true.* For all falsehood and fallacy rests on lie, the will to be false, even in those cases where we speak of error; and this will for falsehood is here completely absent. Thus if we would stick to absolute truth—of course subjective truth is meant, for there is no objective untruth—we could all be inspired, we could all be prophets. Of course a good many things which we are accustomed to say under the present conditions we should not say then at all. *E. g.*, we should never pretend to know anything with certainty about the future, except that which we can deduce mathematically. We should never state as certainty, what is a matter of belief (which we now continuously do, *e. g.*, with regard to causation). We should never claim to "know" the intentions of others, the events of the past and the laws of nature for the future, and however great our knowledge would be, we should never deny that with regard to *action* "we walk by faith (belief) and not by sight (certainty);" we should not identify ourselves with our body and we should not spend so much energy of individuals, corporations and nations in the acquisition of goods of

chiefly imaginary value. We should not make use of ambiguous terms, pseudo-conceptions and pseudo-distinctions. We should not, under the pretense of the promotion of Progress, Freedom and Civilization induce the crowd to commit injustices and cruelties, and to do on collective responsibility what no truthful individual would like to be responsible for. And last, not least, we should not fail to recognize that the much demanded and much professed "freedom of speech" is a blessing only when in necessary correlation with the "Duty of Truth," without which it amounts to nothing less than the "Tyranny of Lie."

It has been customary with many philosophers since the time of Socrates to regard *sin* as the result of *error*. Regarded in the light of the above considerations, I think the truth is just the reverse. *All error is based on sin*, and especially the arch-type of sin, without which no deed is a sin at all, viz., *lying*. If we would take the events of our experience just as they are given, without stating as necessary and actual what is not necessary and actual, without attributing properties to those complexes of states of consciousness called things, which they do not have, without giving induction and analogy more prerogatives than is their due, or, in one word, *if we would never lie, there would be no error*. For, it is not the facts or their relations themselves which contain or constitute the error; it is always the interpretation or statement regarding them. If we make an error in a mathematical deduction we must have somewhere regarded or stated something as certain or necessary which was not so. And it must not be said, that the error consists just in our not noticing where we did this; for we are always aware of it, if we take something for granted, as absolutely certain where, out of indolence or for other reasons, we have omitted careful and thorough inquiry. And if we indeed did not notice it, it is not because we did not commit that sin of omission at that point, but because we committed it also at other points, where, however, the facts happened to be in accordance with our belief, which we carelessly treated as knowledge.

But wherein consists the sin, which is at the base of all errors in ordinary life, in scientific inquiry and even in relig-

ious endeavor? It is chiefly the unwillingness to admit that our field of knowledge, *i. e.*, of absolute (either apodictic or assertive) certainty, is extremely limited and that in all our actions, in all our striving for success and progress, *belief reigns supreme*. We are all too apt to give the one of the few fundamental distinctions of which we are capable, namely that between the *agreeable and the disagreeable*, the preference over the other, namely that between *true and untrue*; we sacrifice truth to pleasure. Man must seek pleasure; it is the very nature of his activity of choosing, preferring. Even if he chastises himself, if he seeks death, he seeks what from some standpoint is preferable, *i. e.*, more agreeable to him. *It can therefore be no sin to strive for the agreeable, for happiness. But we should never seek the agreeable at the expense of truth.* And this is what we do, if we state as certain, as actual or necessary, what at best is a matter of belief. We have become accustomed to this kind of lying through thousands of years of training. We do it not only in ordinary life and conversation; we do it as public men before public audiences, in political discussions, in lecture rooms, and even in the pulpit. We state a thing as true or certain, because we like it to be so. The greater part of our phraseology in ordinary daily life, and a good deal of that of science and philosophy, too, partakes in this system of at least careless lying. This, I might call "hereditary" sin of seeking the agreeable at the expense of the subjective truth has brought about in common life a low materialistic utilitarianism, which identifies man with his body and his noblest aims with the acquisition of property, influence and money. In political life it has created a mutual and almost irreparable distrust between individuals as well as between nations, a tyranny of the crowd and its leader, the unscrupulous catchword-demagogue, a very superficial and unjust press and an ignoble party spirit which makes nations waste their energy in a fruitless tug of war between factions. In the religious sphere, finally, it has produced an unintelligent indifference toward all matters of faith in one class of people and in another class a narrow-minded, self-righteous Pharisaism, which in condemning every one who is not like-minded, is more remote than ever from the Christianity taught by Christ,

which is infinite love and forgiveness; forgiveness to every one and for everything except transgression of the law of Truth. X

So much as regards the philosophical side of the subject. Let us now turn to consider briefly its pedagogical implications.

We have tried in the foregoing to show, that there is nothing unreal in this world except the products of human lying. The first condition for an essential progress in the direction of a greater perfection of the human race will therefore be a greater adherence to the truth than we were accustomed to, hitherto, or, as we might also say, a greater objectivity. But, if we use the latter term, we must not forget, that, though it sounds paradoxical, *the greatest objectivity consists in the strictest adherence to the subjective truth*. It should consequently be the foremost aim of the educationalist to incite the pupil first of all to that thorough truthfulness from which we are so far remote to-day and which is, though perhaps not itself the highest of human and Christian virtues, certainly the *conditio sine qua non* of all. Indeed, if we do not make more earnest efforts in this direction, all our professed Humanity, Civilization and Christianity will remain sham and deception.

Here the question of the educational ideal must be raised. What should our pedagogical ideal be? It is commonly said: The *man of sound principles*, the *strong character*, *harmonious development of all faculties*, or the *perfect man*. Now with regard to the first two of these ideals we claim that they are meaningless expressions, mere empty words, worthless pseudo-conceptions, which cannot stand the analysis of a thorough logical criticism. The only sound principle which I should be inclined to admit is "*to have no principles*" but to act always according to our sense of truth, that spark of divinity in us which we call our conscience and which urges us to decide all questions which involve moral responsibility uninfluenced by desire, habit and imitation, with regard to nothing else than truth. If we adopt as a principle or maxim a proposition of mathematical certainty or necessity, it is superfluous, for our conscience or sense of truth tells us to act according to it anyway. And if we adopt as a principle a proposition which is not absolutely certain, it might turn out later to be false; and then we have pledged ourselves to do wrong in all cases; namely, either to

act against our principle or against our conscience. Similarly with regard to the other pedagogical catch-word "character," if it is intended to mean anything else than "individuality," "individual quality," I claim that *the best character is he who has no character at all*, but who always acts and speaks according to truth. He at least is the only "good" character; all others must be "bad" ones. If some good Christian is not convinced yet of the correctness of this argument about character and principles, I would like to ask him: What character had Christ and what principles did he follow and teach?

And as to the other two ideals, there will be almost as many opinions as there are individual thinking beings in this world as to what is "the perfect man" and what is "harmonious." And so with all other pedagogical ideals. No matter what great perfection is claimed by the word which designates them, they are at least as imperfect as the person who "conceives" them; and if we conduct education in fixed tracks, directed towards such inadequate, vague, and imperfect ideals, we set unjust limitations to possible development and progress.

There is too much of such positive ideals in the educational world, especially on this continent; there is too great a tendency yet toward that mediæval method of education, which makes the pupil go through all the little byways the teacher went through, and thus makes him at best as good as the latter; and which regards him who can best recite his lesson, as best fit for teacher. The result of this method is not a new generation with new ideals—for we do not allow them to form their own—but a generation of average men, with here and there a faddist between, a suppression of all originality, a continuously increasing tendency to think as little as possible and to act only as a member of a crowd.

We should, on the contrary, lead our pupils to avoid the errors and unnecessary byways through which we had to go and try to bring them to a point where they have a horizon broader than ours. We should help them to reach the periphery of our own sphere in the shortest and simplest possible way. We should urge them on without forcing them in a certain direction. We should help them to keep on the straight line, but we should let them choose the direction for themselves.

Thus I claim: We should have *no positive ideals of education*.

We should confine ourselves to the negative, prophylactic and preventative method. We should cut off such off-shoots as are clearly based on error and falsehood. In other words: We should not attempt to force the tree, by trimming, one-sided direction of light and watering, into artificial forms of our own biased imagination. We should remove the dry and rotten branches, but for the rest, we should let it grow as God and Nature will.

Many educationalists are somewhat afraid of originality in the pupil, because they do not know whether it is going to develop into the qualities of a genius or into those of a scoundrel. But if we would educate to Truth and not allow the slightest transgression of that divine law,—I believe that all theories, that lying is admissible under certain conditions are based on fallacies,—all originality which could pass then, would be of the nature of genius.

But I hear the objection: Do we not always teach children to be true, and even punish them severely if they lie? Yes we do, but in a half hearted manner, which bears the untruth on its forehead. If we detect a boy lying, we put a very stern face on, punish him and tell him that it is a great sin. But shortly afterwards he hears grown-up people contradict themselves or state things as certainties of which he can clearly see, that they cannot be certain. Or he hears his parents or teachers or friends speak *of* other persons, what they could never say *to* these people. He is taught to be scrupulously honest, but he soon realizes that grown-up people are not so themselves, that the modern business principle is not to excel others in the quantity and quality of work, but in the smart way of taking advantage of the comparatively greater honesty and innocence of the others and to make the greatest possible profit by the least amount of work, or even by doing no work at all except that of lying and filling one's own purse.

The boy is taught in church and school that all men are brethren, but when he comes home he finds that his parents "cannot associate with such uneducated people as mechanics or laborers." In words he is taught not to be egotistical and to love his neighbors as himself, but by the actions of the people he is taught the contrary; namely, to take advantage where he can, and that his own party, his own creed, his own nationality

is superior to all others. He must see, that almost the whole world hypocritically sides with Armenians and Bulgarians, without ever examining their cause, simply because they happen to have the name of "Christians,"

He is taught that only such competition is noble and worth imitating, as reaches its aim without damaging and hindering the other competitors. We should try to excel even whilst helping the others on. But what he observes around him is chiefly the other kind of competition which succeeds by cunning tricks (*i. e.* lies) and tripping up the rivals.

He is taught that gambling is an abominable vice and he sees that the man who lost his fortune by gambling—even if he gambled honestly—is the object of utter contempt. But he cannot help to notice also, that the successful gambler, no matter whether he made his fortune in Monte Carlo or in Wall Street, is worshipped as a smart fellow and his tithes are welcomed even by the churches.

Whenever a noble aim is ostentatiously proclaimed it is done in the name and for the sake of "Humanity" and "Civilization." But when less noble aims are pursued in the interest of his own nation, party, family or business, and even the most ignoble means are resorted to, it is excused by the phrase: such is "human" nature. He further sees that if some one has done wrong and confesses it or is convicted, he is, in spite of all professed Christianity, never allowed to start anew. He is an outcast and despised forever. But if he succeeds in lying himself out of it, he is a smart fellow. He reads the papers and sees that even the evidence given under oath in court by witnesses and expert witnesses is full of contradictions.

On the basis of such and other experiences the child cannot fail to form his opinion as follows: Truth is a fine and beautiful ideal, indeed it is more, for it is written with ineffaceable characters upon my own heart as the fundamental moral and intellectual law. But it seems that people are not very particular to keep this law which they so loudly profess, and no authority seems to be willing to strictly enforce it. Since I do not want to swim against the current and to be crucified, I shall do as the others do: I shall profess what the people call "Humanity" but I shall act according to their "human nature." I shall

never admit to have erred or to be in the wrong. I shall stick to the truth as long as it is to my advantage; in one word: I shall *profess* truth and *do* as I like. Only children and fools, according to the proverb, find it pardonable to speak the truth pure and simple; and I shall soon cease to be numbered with either class.

We are always inclined to treat children as if they had a very weak intelligence but a strong will. I think the case is just the reverse: The child has usually a very weak will but a very strong intellect. It is for me a great question, whether the so-called mental development of the child is really a development of the intellectual power or only an adaptation of an unalterable intellect to the mode and means of expression to be found in the surroundings and at the disposal of a developing body. This problem can scarcely be solved and may in the last instance turn out to be a pseudo-problem. But so much is sure: most children have at a time, when they can scarcely express themselves in words and gestures, completely grasped the equivocal nature of the teaching of truthfulness, which they get from grown-up people. Apart from the above problem as to the growth of the intellect itself or of its power of adaptation and expression, we are usually inclined to underrate the intelligence of children for two reasons: First, because they do not govern perfectly that external object, which is called their own body; and second because they use language in a way which appears to us "childish." But we must not forget, that the body and its organs, and that very arbitrary and inconsistent system of symbols which we call language, are not the intellect itself but only its inadequate instruments and means of expression.

Summarizing the foregoing I venture to say:

1. There is nothing unreal in this world except the products of human lying.
2. The progress of human perfection depends on the degree to which we succeed in eliminating untruth.
3. We should renounce all positive ideals of education except Truth. We should educate to perfect Truthfulness and leave the rest to God and Nature. For:

Truth alone will make us free.

BINOCULAR VISION AND THE PROBLEM OF KNOWLEDGE.¹

By Professor JAMES H. HYSLOP, Late of Columbia University.

It was the doctrine of Kant that gave importance to the problem of space perception. Had it not been for that philosopher's paradoxical assertions about the nature of space and its perception we should probably have taken this datum of knowledge as a dogmatic object of faith. Previous to Kant no special theory of knowledge or metaphysics depended upon any particular doctrine of space and its perception. But the great Königsberger formed a definite theory of it and drew certain philosophic consequences from it. This theory consisted in a double qualification of its nature. He described it as a "form of intuition," and qualified this as apriori and subjective. That space was a "native" or intuitive perception was the accepted doctrine after Descartes, but none had attempted to describe this "native" perception as subjective until Kant ventured upon the assertion. The consequent idealization of knowledge and reality, whatever such idealization meant, had so many revolutionary implications in philosophic thought that it created much offense in the ranks of common sense and science. Common sense did not like the idealism founded upon it and the scientist did not like the concession to apriori doctrines involved, inasmuch as induction and experience were the watchwords of the scientific movement. Both schools of thought conceived it their interest to attack the Kantian philosophy by depriving it of the foundation which Kant had placed in the apriori and ideal nature of space and time. The scientific man attacked its apriori nature and the common sense philosopher attacked its ideality. Between the two it was

¹It was my study of space perception under President G. Stanley Hall, while at Johns Hopkins University, that first interested me in the problems here discussed and prepared the way for all my later investigations of them.

hoped to eradicate the system. The consequence was a vast literature and direct experimental investigation to determine the issues raised by the alleged significance of the Kantian doctrine of space and time. The fact was, however, that the system was less dangerous to science and less antagonistic to the existing philosophy than was supposed. The real conflict between science and transcendentalism lay in their associations. The one was liberal and the other conservative in its affiliations. Science had attached itself to progress and revolutionary tendencies. Transcendentalism, whatever its sceptical impulses, had easily adjusted itself to the conservative institutions of society resisting change and the dissolution of traditions. As the system depended upon the apriori nature of space and time the scientific mind resolved to remove this keystone to the arch of the structure and consequently directed its investigations to this end, assuming that it had not to discuss any of the larger philosophic problems ostensibly founded upon the doctrine of space and time.

The outcome, however, has not been what was expected. It was thought that the disproof of the apriori perception of space would disqualify the idealism founded upon it, but, as the Nemesis of scepticism would have it, Wundt, an empiricist in the doctrine of space perception, definitely claims that this view affords a better basis for the Kantian idealism than Kant's own conception of its foundation. In fact the solution of the problem has been found not to be so easy as was at first imagined. The complexities and equivocations involved in it suffice to take the dogmatism out of the theories on both sides and now one does not care whether space perception be empirical or apriori. No such philosophical consequences as Kant claimed for it are recognized by any of the parties to the controversy about the *genesis* of our idea of space, but only its *nature*, whatever its genesis. Hence the issue has completely shifted from the psychogonical to the epistemological and metaphysical problem which lies on the boundaries of metaphysical speculation.

The immemorial problem of knowledge has been connected with the question whether we could ever know anything beyond our mental states and affections. In thus defining it I

am not ignorant of the complexities and equivocations of such a formula lurking in the term "knowledge," "beyond," "mental states," etc. I am only stating a formula which at least appears to limit "knowledge" to the functions of the organism or mind in the sense that its boundaries are to be defined by the limits of the organism itself. It is not my task here to define and analyze the formula or determine what is true or false in it, but only to indicate that the result of antecedent conceptions has brought men to formulate their conclusions in this language with its apparent import. How this movement began we shall see in a moment. But long after it had seized speculation the triumphant refutation of scepticism was based upon the accepted integrity, the objective import, of our ideas of space and their impregnability against sceptical analysis and attack. But the Kantian claim that space is "subjective and ideal," whatever Kant may have meant by his terms, reanimated the old controversy, and at least in the light of the traditional conceptions and implications of the terms "subjective and ideal" suggested the limitation of knowledge to states of consciousness in a more radical sense than ever. While previous thought, accepting the relativity of our knowledge of matter, had still remained by the objectivity of space, the new position taken by Kant left the imagination with nothing but the subject and its own evanescent states as the objects of knowledge. What it meant in the field of perception was that we could *perceive* only what we *have* "in" experience; that is, nothing "outside" consciousness, and so at least apparently outside the organism, could be seen or known. The range of the knowable was limited to the states known.

The phenomena of illusions have been the most important influence in suggesting the way in which the limits of perception shall be determined. They indicate that the supposed reality beyond the mental state, which is so confidently assumed in normal conditions, may be nothing more than the subjective act. The resemblance between the illusion and the normal state, between the phenomenal and the real, is so close that the unity between them is gotten by eliminating the "reality" of the normal, the only difference between them being that reality which may be assumed to be inferential and so

liable to error, so that certain knowledge appears to be limited to the subjective. Valid perception seems thus to be realized as fully in illusions as in the supposed normal consciousness, the reality of whose external object appears dubious because it is inferential. Briefly stated again, we can only perceive what we have; knowing and being are identical.

The rise and development of this conception is an interesting bit of history. I mean, of course, in reflective and speculative thought. The whole doctrine got its inception from the naïve materialism of Empedocles which was probably a reflection of common notions at the time. The manner in which Empedocles accounted for sense perception by the impact of the eidola or corpuscular effluvia upon the sensorium, eidola which were the *fac simile* of the objects from which they were projected, appears to us absurd enough, especially from the point of view of evidence, but it illustrates clearly the assumption that there is some qualitative resemblance between the "impression" and the stimulus or cause of sense perception. The figure of the seal and the wax, even in Aristotle, carried the same implications with it and probably affected the conceptions of antiquity to a large extent. The Greek admitted the distance of the object from the sensorium, but could not account for the knowledge of it without importing into the problem the conception of contact with the subject and a structural resemblance between the object and the "impression;" that is to say, though knowledge was not limited to the subjective state, there was some kind of identity between objects and knowledge, the "reality" and the "impression" were similar, the intermediate distance between them being traversed by eidola resembling both of them.

But this corpuscular theory was very soon supplanted by the doctrine that it was not eidola but motion that passed from the object to the subject and served as the stimulating cause of sensation. Here the whole conception of the case was changed. In the Empedoclean view the assumption of identity between "impression" and eidola, and between eidola and object, sufficed to justify the belief about the nature of the object. But in this new view, depending upon the mediating and causal agency of motion, there is no definite indication at first that

motion and object were like each other. In fact it was rather distinctly assumed that they were different, and as the older conception of the object still prevailed the analogy of the seal and wax did not apply. The inevitable tendency of the new conception was to set up an antithesis of kind between some of the data involved in perception. There were three things to be considered: object, motion and impression. Until Plato came to revise the problem the motion was not like the object, and the problem at once arose to determine how the external object, separated from the impression and unlike the mediating cause, could be known. The consequence was that perception was limited to the sensory state, whatever that was, and the supposition was that this was no indication either of the fact or the nature of the object. Hence we see the logical outcome in the doctrine of the Sophists which was reinforced by the general relativity of knowledge, this being based upon the fact of illusions as well as the assumption of contact with the organism as a condition of knowledge. The Sophist still assumed the identity of "object" and "impression" (thought and reality), but he did not locate the object beyond the subject in his conception of the thing "known." There was an antithesis between the "object" as external and the "impression," but this "object" was *nil*.

The most important thing to remark at this juncture of the matter is the fact that later thought never returned to the naïve conceptions of Empedocles in order to render the process of perception phenomenally intelligible. The speculative philosopher felt obliged, in the field of vision, to abandon the conception of contact as an explanation of perception and consequently had a perpetual puzzle before him in the question, "How can we perceive what is not consciousness, or in contact with the organism?" How can objects at a distance be known at all? Presumably they are not so known in tactual experience, which is the most fundamental source of our conception of "sensation," according to the usual assumptions, where vision is predominantly the *perceptive* sense and touch the measure of *sensation*. In tactual experience the supposed external object and the sensation have the same locus, the sensorium or organism: in vision the common assumption is that the object is not in

contact with the sensorium and the very existence of a sensation is an inference from the experience of touch. But as the motion (vibration in modern parlance) which is supposedly emitted from the object does not represent the object in kind, but does satisfy the principle of contact, according to the accepted view, in visual experience, and as tactual experience and the assumptions associated with it determine the tendency to interpret sensation as functionally limited to the locus of the sensorium, the inevitable tendency is to interpret visual phenomena in terms of the principles assumed in touch, the object being and acting where it is, or is not seen at a distance, so that in sight the same conception comes to determine the mode of conceiving its phenomena. Apparently we seem forced to interpret vision by touch or touch by vision. If vision is to be explained by the assumptions of tactual experience, these being conceived in terms of contact, visual perception has the same limitations. If touch is to be interpreted by the analogies of vision where the object is supposed not to be in contact, we come into conflict with the fact that we do not perceive the tactual object at a distance. The consequence is that we get our unity of thought and conception in the general idea of sensation which limits its nature and meaning to the area of the sensorium; and the object must be there to be known, or if supposed to be at a distance, it is apparently a conjectural thing. Now as the principle of identity had all along been assumed to determine all intelligibility in experience, this new assumption of an antithesis between thought and reality, of difference between sensation and the object, if it existed and was not present in consciousness spatially or temporally, only availed to make an object an unintelligible notion, in all conceptions of it as at a distance, with motion as the mediating agency for affecting the sensorium. In other words the tendency arises to interpret vision by the assumptions and conceptions of touch, which involves contact as its standard of judgment; and to consciousness the object at a distance is *nil*, or conjectural. Consequently the definite knowledge of vision was, like touch, limited to the states of the sensorium. Or to put the same thought in another way, what is not a qualitative part of the "impression" cannot be "known."

This conclusion brings us to the doctrine of Berkeley who seems to have been under the influence of assumptions which he did not analyze. His whole discussion of space perception was governed by the supposition that what was not "in" the sensation or impression could not be perceived, or that we could perceive only what was in the sensation. This doctrine was embodied in the formula "esse is percipi."

The most important fact to note in Berkeley's position is his argument to exclude the nativity of the visual perception of the third dimension. The argument used by him against the organic and natural perception of distance in vision was that the third dimension was not found in the image on the retina. At the very outset of his "Theory of Vision" he says: "It is, I think, agreed by all that distance of itself, and immediately, cannot be seen. For, distance being a line directed endwise to the eye, it projects only one point in the fund of the eye—which point remains invariably the same, whether the distance be longer or shorter." In a later section he says: "It is plain that distance is in its own nature imperceptible;" again: "From what hath been premised, it is a manifest consequence, that a man born blind, being made to see, would at first have no idea of distance by sight; the sun and stars, the remotest objects as well as the nearer, would all seem to be in his eye, or rather in his mind." These quotations suffice to show that Berkeley thought the presence of the third dimension or solidity in vision was necessary to its immediate perception by that sense. The plausibility of the assumption rested upon the supposed fact that plane dimension was found in the retinal image precisely as conceived, while it was clear from the law of optics in the transmission of light and the production of images that no solidity was present in the impression. The assumptions of touch and contact were his principle of interpretation and carried with them his doctrine of "esse is percipi." But when he came to discuss the perception of plane dimension he denied its nativity on other grounds than the absence of it in the retinal impression and virtually abandoned the assumption which was so necessary to the validity of his argument regarding solidity. He based the denial of the nativity of magnitude or plane dimension upon the relativity of the perception

of it, that is, upon the quantitative variations between the dimension of the image and the dimensional quantity of the object seen. But as his argument against the native perception of solidity was based upon the assumption that, to be known directly, it must be in the image, he ought to have seen that the admission of plane dimension in the retina, whether quantitatively identical and corresponding to the dimensional quality of the object or not, was necessarily a guarantee for the nativity of the space percept in plane dimension, so that the facts to which he appealed to disprove it only showed a quantitative difference between the retinal quale and that of the object. In fact, it was logically necessary to admit or assume the nativity of plane dimension in order to make the fundamental argument good against the nativity of the third dimension. For if that were not true there was nothing to prevent the supposition that solidity was native in spite of its absence from the retinal impression. But since the assumption of plane dimension in the retinal image, according to the use made of it in regard to solidity, enforces a conclusion which is contradicted by the conclusion from the relativity of magnitude, as drawn by Berkeley, and since his doctrine denied the nativity of space perception throughout vision, we can only conclude that this denial had to be maintained independently of the question whether the retinal image contained the dimensional quale perceived. The abandonment of this point of view, however, indicates either that his fundamental assumption was not valid or that his consistency required him to admit the nativity of plane dimension in spite of the differences between the image and the dimensional quale of the object, at least in quantity. For he must admit either the nativity of plane dimension or that its presence in the image does not determine its perception. The former alternative contradicts his doctrine, the latter contradicts his assumption necessary to prove the acquired character of the third dimension in vision. Now if the presence of the dimensional quale in the image does not necessitate its natural perception, its absence from the image certainly cannot prevent the perception of it directly. This is the necessary consequence of the argument adopted by Berkeley, and it means that we cannot assume that the quale known is neces-

sarily a part of the content or nature of the "impression." That fact once granted the whole Berkeleian doctrine is groundless. It will be apparent from such a result and from the supposition that the percept may not be a part of the "impression" that the doctrine of perception as conceived by the phenomenalist and idealist must be profoundly affected thereby, whether for good or ill.

We know that Berkeley explained the visual perception of space by association, or suggestion from muscular and tactual experience. But it never occurred to him that it was quite as easy to raise the sceptical question in regard to the nativity of space in touch as in sight. Of course, he was not likely to suspect this, as his assumption of the principle of contact and the representation of the quale perceived in the impression induced him to accept tactual space without analysis or scepticism. It was all very well for a paradoxical philosophy to beg the question in one of the senses while applying criticism in another. But the fact is, it appears to me, that there is no more reason to suppose that space is native in touch than in sight. Berkeley's argument may puzzle those who cannot have the last word with a philosopher, but it does not disturb the equanimity of those who feel as capable of deciding what they see with their eyes, whether subjective or objective, as they are of deciding what they can feel with their hands. Of course, we may neither see nor feel anything. I shall not deny a consistent scepticism. But I should not be troubled any more with the phenomena of vision than with those of touch. I agree that there is a quale in touch that becomes associated with another in sight, that a certain fact in vision will have a certain associated meaning in touch. But that they should be identical is to admit the presentation of the same datum in both senses, a doctrine which it was Berkeley's object to deny, namely, not only the nativity of space in vision, but also the view that a quale could be perceived which was not in the impression.

Now whatever we may think of Kant's doctrine of space and its perception, it is certain that he cleared up a great deal of confusion by asserting the ideality of space. He did not enter into any analysis of the percept in relation to the different

senses. Whether he should have done so or not it is not necessary for our present purposes to inquire. It is simply the fact that he did not investigate the problem whether or not it involves a synthesis of several sense perceptions with an abstraction of their common content. He in fact denied that it was an abstract idea or general concept. But his general doctrine that it was subjective and ideal as well as *apriori* was the most radical limitation of perception to what was in the impression that had been made. He put forward no paradoxes like Berkeley to prove his system. He simply asserted its ideality and allowed the logical trend of philosophy to accept it without specific or experimental proof, and it cut up by the roots all motive for any other perception of space than what could be affirmed of any other quality of experience. Nothing could be seen which was not presented or represented in the sensory impression or the act of consciousness.

With this outcome of the development of the problem of perception let us see how the phenomena of binocular vision affect both the Berkeleian and the Kantian doctrines. There are just two things to discuss in it. They are (1) the question of its nativity, and (2) the question of its ideality.

I shall confine my discussion of the first of these questions to the problem of solidity or the third dimension in the field of vision. I shall assume, for the sake of argument only, that plane dimension is present in the retinal image, and assume that the absence of the third dimension in it creates a perplexity in the perception of solidity. It was all very easy and plausible for Berkeley and his followers to try to explain this perception of the third dimension in vision by the association of tactual and muscular experiences with certain signs in vision, as they assumed the necessity of the presence in the impression of the *quale* to be naturally seen if it were to be supposed a native function of that sense. But Brewster's and Wheatstone's work on binocular vision, showing that the perception of the third dimension was connected with the existence of disparate images on the retina, suggested the existence of an organism for the native perception of distance which Berkeley did not suspect, all this work having been done after his time. We know how this led to the invention of the stere-

oscope and what this instrument was designed to illustrate. This was the production artificially of disparate images on the retinas for the purpose of eliciting the perception of solidity. The same effect also we know from the multitude of experiments in artificial combination of images by the use of the naked eyes, all of them illustrating the effect of the fusion of disparate images on the retinas when there is sufficient resemblance between them to effect this without too much rivalry. Instead of repeating any of the facts here which I wish to use for argument I shall simply refer the reader to papers already published. The reader may consult the following: *Mind*, Vol. XIII, pp. 499-526; Vol. XIV, pp. 393-401; Vol. XVI, pp. 54-79; *Psychological Review*, Vol. I, pp. 257-273, 581-601; Vol. IV, pp. 142-163; pp. 375-389 (this last reference is to Prof. Judd's article). Helmholtz, *Physiologische Optik*. Le Conte, *Sight*. I might also refer to the work of Hering, Aubert, Wundt, Stumpf, Lipps, and Martius.

The experiments recorded and described in these references exhibit the fact that geometric figures can be so drawn as to produce binocular parallax similar to that of solid objects on the retina and that the effect on the perception of distance or solidity is the same. They are simply variations of the phenomena of stereoscopic vision. Now Wheatstone showed with sufficient conclusiveness that the perception of solidity was associated with the existence of disparate images from solid objects and these diagrammatic experiments referred to above show the same fact with variations in a manner to indicate that there is a native function in vision to perceive the third dimension, a function at least apparently distinct from every form of association and inference. Whether it is properly so or not I shall examine presently. But what I wish to note first is the fact that this solidity is not present in the image on the retina. We may say that it is *represented* there by the binocular parallax or disparate images. It is true that there is something in binocular images different from the monocular, but this difference is not identical with the difference between plane and solid dimension. The difference is purely a matter of parallax in plane dimension or magnitude: while the perceived quale represents the third dimension. In such cases we un-

doubtedly *see* what is not in the "impression." There is no representative correspondence between the "sensation" and the quality seen. Its nativity is apparent in the fixity and uniformity of the phenomena, and the variations in a manner contradict the theory of association which ought to make the result capricious and variable. That is, if association and inference be the interpretation of the phenomenon, the perspective or solidity ought to involve localization as alterable as it is in monocular vision where geometric figures can have their form and apparent solidity seen pretty much as we will. Take the case of the geometrical cube as an illustration. We can see the cube in more than one position, if we think of the way we wish to see it. Also geometrical figures representing a tube or tunnel, which may be made to appear with the small end nearer or farther from us, as we wish to see it. But this phenomenon does not occur in the experiments with binocular perspective, unless we destroy the fusion and reduce the perception to the monocular. The organic character of it and the variation of the solidity according to the laws of fusion show that it is natural. I do not care what may be said of its evolution. Anything may be granted here. I am concerned only with what it is now in the experience of the human race. This is that there is an organic function for the perception of solidity in vision without having this *quale* present in the image.

I must call attention to an interesting difference between the experiments with geometrical figures and the facts of perception in normal cases of solid objects. There are two facts to be observed in normal binocular vision in the "impression," one of them involving apparent variations from purely geometrical considerations. There is first the purely geometrical disparateness of the images produced by solid objects. This is the same as in geometrical figures. But the second fact is that in actually solid objects the parallax involved in the disparate part of the images is accompanied by some slight difference in the intensity, relative or absolute, and mathematical perspective, as compared with the common part of the images. This might be said to be an important factor in the clearness of the third dimension in visual perception. While I admit that

it may affect either the judgment, supposed by the associationist, or the perception supposed by the perceptionist, it is evidently not the decisive factor in the case, because in the experiments with geometrical figures this difference of intensity in light and the existence of mathematical perspective are absent and the perception of solidity is apparently quite as clear as in actual instances of solid objects. That is to say, the perception of the third dimension is not apparently affected by any circumstances but that of mathematical disparateness and parallax, so that inferential factors supposedly associated with variations of intensity and mathematical perspective are excluded from view. In the experiments, therefore, with geometrical figures we have the clearest evidences of the nativity of the perception of distance, against the claim of Berkeley, without the presence of that quale in the image or impression.

The associational theory is easily disposed of with the remark that there is no reason for denying that tactual and muscular space become associated with the visual quale which I have been discussing. But this fact does not involve any identification of the tactual and muscular quale with the visual. What I am discussing is the visual quale *seen* directly and not its inferred or associated correlate in experience foreign to sight. We may very well discover by experience that a certain visual fact is associable with a certain tactual and muscular fact, not identical with it as a presentative percept, though a space content be in both. Hence I deny the associational theory by admitting it, while refusing to accept its relevance to the problem before me, which is not whether the visual quale has no tactual and muscular correlate, but whether there is not a visual percept that may be called the third dimension in that sense whether interpretable or not in equivalents of other types of experience. The visual third dimension has its correlate in tactual and muscular phenomena, but it is not constituted by them. The reason that confusion arises is that vision is our anticipatory and touch is our protective sense. This fact always makes it necessary to interpret our visual experience in tactual and muscular terms or correlates as a means of regulating our volitional actions and adjustments. But this utilitarian consideration in the process of development does not in-

terfere with the nativity of the visual space quale any more than the associability of a taste with a color proves the empirical character of the latter.

If "experience," association, and "motor" phenomena are to be entitled to any consideration in the case, so far as my conception of the problem is concerned, they must be confined to the sense of vision whose data alone I am discussing, and simply for the reason that an associable tactual and muscular correlate is admitted in the case but refused the right to be considered the whole of the phenomenon. It is clear that within the sense of vision association does not determine the result, and any other association is irrelevant when true. That distance is a "motor" phenomenon in vision does not alter the contention here made, namely, that the perceived quale is not, as perceived, a part of the retinal impression. You may interpret "motor" phenomena any way you please. I am not concerned with the interpretation or with the truth or falsity of that doctrine. The position that there is a visual quale for the third dimension is wholly independent of that controversy.¹

¹ My own position on the "motor" theory of space perception has not been altered in the least by anything that I have seen before or since my own experiments were published. Prof. Judd (*Psychological Review*, Vol. IV, pp. 375-389) confirmed the fact in his repetition of the experiments and added some experiments of his own, and then remarked that they proved the "motor" theory which I had rejected, and he identified the "motor" theory with that of association, indicating that he was following in the line of the assumptions involved in Berkeley's use of muscular experience. But Prof. Judd neither attempts to show how my experiments and his prove the "motor" theory, nor defines what he means by that doctrine. He simply asserts that they prove it. If counter assertion would suffice I would only say that all his experiments simply confirm the position I had taken instead of disproving it. The real difficulty with my critic and this whole school is that they do not define their position in relation to the one I was opposing. I had indicated clearly enough my position in *Mind* (Vol. XIII, p. 524) and this involved the long standing and accepted distinction between sensory and motor functions of the nervous system, a distinction still fundamental in physiology and un-abandoned, except as the psychologists have come to use the term "motor" to denote, not the agencies initiating muscular activity as it originally meant, but the sensation of motion, a fact which is not antithetic to sensory functions at all. The apparent novelty of the

On any conception of "motor" sensations, whether the function of "motor" centers is distinguished from the sensory, or whether they are merely sensory facts involving the consciousness of motion, the quale perceived as a result of binocular parallax is not presented in the image, and this fact is sufficient to prove that the visual percept is not similar to the datum in the sensory impression.

This conclusion is very distinctly confirmed by the phenomena of upright vision, and in a manner which absolutely prohibits the influence of association with tactual and muscular experience. We know that the retinal image is inverted and that nevertheless objects are seen in their proper position. Experiments with a light thrown upon the retina through the sclerotic coat of the eye show that the directional reference of vision is functional and explain why objects are seen in their correct position in spite of the inverted image, and this position is not determined by any principle of vision requiring perception to reproduce the relations in the retina in its judgment of reality. We see objects as they are apparently without any identity between the image or impression and the reality.

It would appear from this conclusion that we may have objects of consciousness which are not "in" consciousness and that perception may "transcend" the states and affections of the sensorium. I do not mean by this conclusion to dispute the idealistic theory of perception. That doctrine is indifferent to what is maintained as the result of binocular experiment. All that I am emphasizing at present is the discrepancy between the retinal and sensorial image and the dimensional quale perceived. Assuming what we know of optics to be true this

"motor" theory comes wholly from the adoption of a terminology which had traditionally implied an opposition to sensory, but which in its new application was identical with the sensory and so lost all power to controvert any sensory theory. I have no objections to the "motor" theory as thus conceived and it does not in the least controvert the claims that I had put forth in dispute of the "motor" theory defined in terms of the physiological distinction between "motor" and sensory functions, one of which was unconscious and the other conscious. This is the reason that I have not been moved by any of the elaborate reversions to so-called "motor" theories. Cf. *Psychological Review*, Vol. X, pp. 49-51.

quale is not present in the impression, but it is distinctly perceived, and though the whole process be "ideal" or subjective, there is nevertheless the difference between what is present in the impression on the sensorium and the percept, a view which lends at least apparent support to the dictum that perceptive consciousness transcends the subjective in its determinations. It is apparent how such a conclusion affects the whole doctrine of knowledge as formulated by that school of idealists who insist upon expressing themselves in language apparently implying that we cannot know anything other than our mental states. Whatever it means, it is certain that we can express the phenomena of vision which are under discussion only in language implying that we see what is not "in" the impression. We seem thus to establish the doctrine of realism in the problem of knowledge.

But the idealist can put in a most interesting reply at this point. He can call attention to the fact that this very discrepancy between the impression and the percept is evidence that the quale is purely a mental construction. This involves the question of the absolute ideality of space, and the phenomena and experiments under consideration may be quoted as proving this fact and as showing the correctness of Kant's doctrine while it indicates the error of Berkeley, at least in the assumptions he made and the method of conducting the argument. If the mind supplies this quale not in the retinal image, the transcendency of the impression may not prove that consciousness is transcended in this percept, as it may be said that the percept is a construction of the mind and not the positing of a reality "outside" the subject. Consequently we would seem to have proved idealism instead of refuting it.

I am not interested in disputing such a claim for idealism. I am quite ready to admit this ideality of space, including plane dimension, and so to accept the doctrine of Kant, only I would contend for the possibility that Kant's conception of the matter may not be what it is usually assumed to be. We may grant that binocular parallax gives rise to the mental construction of the third dimension, yet there is nothing in this fact to prevent the supposition that the construction correctly represents an objective fact. That is, the ideal construction may

have an objective meaning though it has a purely subjective genesis not in the impression. It is the analogy suggested in "subjective" sensory impressions that limits the import of phenomena so described, but if the perceptive act apprehends or creates a quale not in the impression, the distinction thus involved and necessitated by the facts opens the way to the possibility that the percept, though of ideal origin, may have an objective meaning, and the only thing that the psychologist would have to do is to show that there is evidence of that fact. Transcendency of any sort having once been established, its limits must be defined before we can dogmatically assert that perception is characterized by the same conditions as sensation, and if they are not equally defined it is only a question of evidence to determine whether its meaning does not involve more than that of sensation. That is to say, is it not possible that the mind is adapted to construct a quale which represents the actual facts in the external world though these facts are not presented in the impression?

Now Berkeley and Kant admitted the existence of "objective" facts of some sort. Berkeley called them "spirits," and Kant assumed them to be other individual centers of consciousness, social persons. This Kant did in spite of the real or supposed radical ideality of knowledge. There is, therefore, in this admission especially, that it asserts the similarity between subject and object, the possibility that space construction only reproduces the quality of external reality, a conception rendered all the more conceivable from the discrepancy between sensation and perception. But for more suggestive evidence we may recur to the doctrine of evolution. In this we find that there is a tendency of individuals to adjust themselves to environment. In some cases this even takes the form of originating positive resemblances in the subject to qualities in the object. This is specially noticeable in color adaptation. In this phenomenon we observe that the color of an animal may gradually change so to become the same as the color of its environment. It is thus quite possible that evolution might develop in consciousness the capacity of ideal action which would represent correctly the nature of objective reality and not present a fact in antithesis to that reality.

But it is the phenomenon of upright vision that offers the most distinct evidence of the possibility or fact which is here suggested. We have seen that the retinal image of objects is inverted, that is, the relative positions of points in retinal images are the inverse of what they are in the objects which are supposed to produce them. We do not have to go beyond the ideality of these "objects" to recognize this fact. It is a fact on any theory of "objects." A double interest attaches to it. There is a radical difference between the sensory impression and the percept, and the percept reproduces the objective relation and not the subjective. However much ideality we assign to the act of perception in this case it reports the objective fact and not the subjective. What is additionally interesting also is the circumstance that the reproduction conforms to the tactual and muscular quale, so that we might even claim that the visual and tactual data are the same in kind, and thus add an evidence to the nativity of visual space while we sustain its objectivity in spite of its ideal origin. Possibly a further vantage ground could be obtained by suggesting that our conception of the nature of the image or impression is indirectly secured by inference, so that the very assumption of what is subjective may be the wrong point of view with which to start. But this point need not be urged. The main fact of interest is the adaptation of perception to the objective conditions.

A CRITIQUE OF 'FUSION.'

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Early in the last century J. F. Herbart declared that the current psychology of his time suffered from three grave deficiencies; it regarded mind as an aggregate, rather than a system; it failed to find the bonds of union among mental events; and it overlooked, in its analyses, the concurrence of mental activities. Reflection upon Herbart's own system of psychology impresses one with the fact that it was just these deficiencies that Herbart was most concerned to supply. He emphasized the organization of mind as against the aggregation of faculties; he insisted that conscious experience is a coherent system of states or conditions; and he set forth typical groupings—complications and fusions—into which ideas fall.

Different as are the concrete methods and problems of our own psychologies, Herbart's system may fairly be said to express the spirit of modern psychology. This cannot be better illustrated than by an historical and critical account of the concept of 'fusion' which owes its psychological application to Herbart.

There are three reasons why a discussion of fusion is, at the present time, desirable. The first of these is, that the enormous amount of psychological analysis of the last twenty years throws into prominence any concept that promises to bring order and organization into the multitude of the more elementary phenomena of mind; the second is, that the term 'fusion' is woefully ambiguous, possessing almost as many shades of meaning as there are psychological systems; and the third lies in the demand for a systematic setting to a mass of new experimental results.

I.

Although various mental phenomena which are now-a-days classified as 'fusions' were familiar to psychologists before Herbart's time,—we find mention of them even in Aristotle,—

the psychological use of the word 'fusion' was extremely rare. In Herbart's system the term springs into prominence through his attempt to bring the empirical data of mental experience—*Vorstellungen*—into relation to his philosophical conception of mind as a simple unitary being.¹ Without opposition, the contents of a unitary mind must be all one, lacking internal distinction; but opposition, if it be complete and unmixed, is no less incompatible than coalescence whether with the facts of experience or with a unitary soul. Now reconciliation of unity and opposition is compassed only when antithesis is counter-balanced by an act of fusion (*Verschmelzungsact*). According as ideas are more or less similar, is opposition, antagonism, more or less in abeyance, and fusion, at the same time, more or less complete.

The most important features of Herbart's doctrine are its metaphysical implications and its conception of fusion as an hypothetical act or process by means of which opposing ideas are welded. The theory is worked out most elaborately for tones. Herbart's whole theory of musical relations is historically important because it attempts what was in his time a decided novelty—a psychological explanation of tonal relations.² But quite apart from general difficulties in applying his principles of likeness and of opposition, Herbart's deductions in the sphere of audition stand at wide variance with the facts.³ Degree of fusion is not, as he maintains, a function of qualitative (pitch) likeness neither does introspection reveal any specific *act* of fusion. From these fundamental errors spring a hundred minor fallacies, which invalidate the whole theory. Herbart's attempt to bring psychological system and order into the chaos of musical theory was wholly sincere and even heroic; but he started from wrong presuppositions, and whenever these came into conflict with the facts, the facts were sacrificed and the presuppositions maintained.

¹Herbart's *Saemmtliche Werke* (Kehrbach's edition), ii, 210; iii, 102; iv, 374; v, 308, 324.

²For a searching criticism of Herbart's theory of fusion, see C. Stumpf, *Tonpsychologie*, ii (1890), 185 ff.

³In commenting on Herbart's 'complete opposition' between a tone and its octave, Stumpf remarks that, in this case, "opposition exists only between Herbart and the facts."

Herbart's doctrine of fusion has been modified in various ways by more recent writers. T. Waitz discusses fusion quite in the spirit of his master, Herbart,—forsaking, however, mathematical deductions, and introducing somewhat freely psychophysical concepts.¹ K. Fortlage makes the amalgamation of like ideational contents one of the four general attributes of the idea.² His derivation of fusion by means of the consolidation of simple ideas is very much after the manner of Herbert Spencer's derivation of the idea from the 'cohesion' of 'vivid feelings' with like 'faint feelings.'³

Volkmann's treatment of fusion is more important. He makes fusion the union to a single act of a plurality of ideating activities. Like Herbart, Volkmann finds the reason for fusion in the simplicity of the soul.⁴ He is at pains to resolve the apparent contradiction in his master's synthesis of opposing ideas. Since unlike ideas must retain each its own identity, they cannot fuse to a single idea. But it is different with the *acts* of ideation (*Vorstellen* as distinguished from *Vorstellung*). The *acts* are not qualitatively opposed. They merely check, inhibit, each other; and their residua fuse into a single act (342). The explanation is clever, but really futile; for the inhibition of an ideating act (*die Aufhebung oder Verminderung des Bewusstwerdens einer Vorstellung*, 345) can mean only quantitative, *i. e.*, intensive diminution, and intensive diminution is entirely different from qualitative fusion which is a matter of clearness—as Volkmann, indeed, admits (346). He, confuses, nevertheless, fusion with the indistinguishableness of

¹ *Lehrbuch der Psychologie*, etc. (1849), 85 ff.

² *System der Psychologie*, i (1855), 127 ff.

³ *The Principles of Psychology*, 3rd ed. (N. Y., 1897), i, 182.

⁴ "Gleichzeitige Vorstellungen verschmelzen, d. h. ihr Vorstellen vereinigt sich zu einem einheitlichen Acte; ihr Vorstellen fließt zusammen zu einem Bewusstsein." *Lehrbuch der Psychologie* (1884), i, 340; cf. *Grundriss der Psych.* (1856), 103. Although V. explains fusion in terms of the simple soul, he professes, nevertheless, to derive his proof of the latter from empirical facts (*Lehrbuch*, 65 and 340). The proof rests, however, upon the assumption that every *Zustand*—every actual mental phenomenon—implies a *Traeger*, a substrate, in which it inheres. His fusion is broader than Herbart's, for it includes 'complications.'

weak impressions. An important issue of Volkmann's theory is that, since diverse qualities maintain themselves under fusion, the fused mass (*Gesammtvorstellung*)¹—although it is a single act—includes a plurality of parts (364).

The most modern defense of the Herbartian doctrine of fusion is made by Theodor Lipps. In Lipps' theory reappears the notion of two antagonistic tendencies in mind. The tendency toward self-maintenance derives from the idea itself; the tendency toward coalescence from the "limitation of mental force," a compression of ideas into a narrow 'psychical space.'² Now degree of fusion is the result of warfare of these two tendencies. Lipps departs from Herbart in one important particular. Instead of deriving fusion from the ideas themselves, he derives it from the capacity of the mind as a whole. The mind is the vehicle of a certain limited amount of force. This force is lent temporarily to a group of ideas, and can be utilized to bring these ideas to consciousness only on condition that they 'stand close' and share the limited amount of energy at their command.³ On the other hand, the conception of fusion as a means of salvation of the unitary soul is entirely Herbartian in spirit (474). A number of 'excitations' have their respective 'rights' to conscious existence (depending upon their respective

¹It is significant that Volkmann identifies his *Gesammtvorstellung* with the 'complex idea' of the English school. At times, he has clearly in mind the epistemological function of this idea, as when he says that "total ideas are those groups of sensations of different classes by means of which we think the individual things of the external world." *Grundriss*, 103.

²*Grundtatsachen des Seelenlebens* (1883), 159, 472 ff. "Seelische Kraft" is, at bottom, an explanation in mechanical terms of the 'active' aspect of attention. Lipps' theory of fusion, which is couched in spatial and physical metaphors, is an interesting study in the psychology of types. Its 'limitation' is the limitation of visual and motor imagery. The theory is a picturesque restatement of mental facts in terms of force and magnitude. Cf. *Philos. Monatsh.*, XXVIII (1892), 547.

³The germ of this distinction is, however, to be found in Herbart's 'Verschmelzung vor der Hemmung' and 'Verschmelzung nach der Hemmung.' The former is a function of the qualitative moment in ideas, the latter is determined by the unity of the soul. *Werke*, v, 324; cf. Volkmann, *Lehrbuch*, i, 371.

'energies'); nevertheless, it is impossible for them to come into their full conscious rights on account of the limited supply of energy at the disposal of the mind. The soul is saved from its dilemma if "the *a* [the several ideas of a group, *e. g.*, of tones] can be made to sacrifice their independence and to fuse. All the *a* are thus brought, in a way, to their rights and yet, at the same time, only so much mental force is expended as a single idea demands." The degree to which ideas may be persuaded to relinquish their claims to conscious existence depends upon their likeness,—like ideas fusing most closely,—upon their intensity, upon attention, and upon practice.

For Lipps fusion is of two kinds, 'total' and 'continuous' (*stetig*). In the former, fusion is a merging of a plurality of qualities into one,—a loss of independence. In the latter, it is the gradual transition from quality to quality which underlies either spatial perception or the apprehension of temporal position. In regard to total fusion—which is also called 'qualitative'—it is only necessary to note that fusion does not indicate any typical connection among ideas. It is simply the conquest of coalescence over independence. Continuous or gradual fusion is a different matter. It is the conscious bridge between qualities set either in spatial or temporal patterns. In Lipps' system, it stands, first, as the means for creating *space* out of slight qualitative differences in tactual and visual sensation. It is an adaptation of Lotze's theory of local signs. Suppose that there are given simultaneously three pressures, *a*, *b* and *c*, so similar that *a* would, under other circumstances, fuse totally with *b* and *b* with *c*, but so different that *a* and *c* would not entirely lose their independence. The three elements will form a spatial continuum of which *a* and *c* are the termini; *i. e.*, the coincidence of 'total' fusion and of slight qualitative independence gives rise to a 'continuous' fusion which underlies the perception of space.¹ Secondly, an analogous fusion of *time*,

¹Lipps' theory of space suffers from the erroneous view that the psychological element of space is the localized point from which are to be derived in order the line, the surface and the solid:—a sheer confusion of geometry and psychology. His theory follows directly from his Herbartian principles, on the one hand, and, on the other, from the pioneer works of Johannes Mueller, E. H. Weber, Fechner and Lotze.

to which Lipps devotes but little attention, proceeds from the rapid succession of like qualities.

H. Ebbinghaus' conception of fusion is best understood in the light of Lipps. Fusion, for Ebbinghaus, means Lipps' 'total fusion,' but it is transferred from the mind to the nervous system. Nervous processes fuse, and give rise in consciousness to a single content which may, however, be broken up, analyzed, by attention. As analysis proceeds, fusion disappears. For the various degrees of unity among tonal intervals (see below Stumpf's definition of fusion), Ebbinghaus proposes a specific explanation in terms of peripheral processes in the organ of hearing.¹

Among the psychologists whom we have thus far considered, we find the same general conception of fusion. From Herbart to Lipps and Ebbinghaus we trace what may be called the traditional use of the term. When, however, we turn to C. Stumpf's interpretation of fusion, we note a radical change in the employment of the word. Stumpf's fusion is much narrower than that of the Herbartian school. It is neither a coalescence of conscious processes in general nor is it equivalent to confusion or to a lack of clearness that disappears with attention or practice. It is a 'sensuous moment' given once and for all with the sensation qualities. The peculiarity of this 'sensuous moment' is that it brings the sensations in which it inheres into a more or less close unity. That is to say, fusion is of various degrees. As Stumpf defines it, fusion is a relation (*Verhaeltnis*) of simultaneous sensations, by virtue of which sensation qualities form not only a sum but a whole (*Empfindungsganzes*); a relation which renders the impression of simultaneous sensations more like the impression of a single sensa-

¹The explanation revives an old theory of Ernst Mach's. It rests upon a modification of the Helmholtz-Hensen theory of audition, for it embodies an extension of the principle of resonance and a liberal interpretation of the doctrine of specific energies. It is not entirely satisfactory as an explanation of Stumpf's type of fusion, because it seems to place degree of fusion more or less at the mercy of relative intensities; whereas fusion, as a 'sensuous moment,' is independent of intensity. Cf. H. Ebbinghaus, *Grundzüge der Psychologie*, i (1902), 326, 481, 573.

tion than of the given sensations in mere temporal sequence.¹ Stumpf compares the fusion to the unity obtaining among the attributes or moments of a single sensation. Just as quality and intensity, or quality and extension, form parts of an inseparable whole in sensation, so do sensation qualities form parts of an inseparable whole in fusion. Although Stumpf confines his discussion almost exclusively to tones, he acknowledges fusions not only in other sense modalities but also between qualities from different senses, as the fusion of taste with smell or with temperature. The first important feature of Stumpf's fusion is, then, its sensuous nature. It is as much 'given' to mind as are the qualities themselves. The second feature is the plurality of fused parts. For Herbart, Lipps and Ebbinghaus, the typical fusion is the merging of qualities into a single quality, or the analogous physiological consolidation; but Stumpf's fusion, on the contrary, always includes a perception or a 'judgment' of multiplicity. In the third place, this last type of fusion depends solely upon the qualitative moment of sensation.² Attention does not affect it; intensity does not affect it; it changes neither with practice nor with analysis. From these three differentiating factors, the identification of fusion with consonance—so far as consonance is a matter of sensation and not of feeling—almost inevitably follows. The identification is more than hinted at in the first volume of the *Tonpsychologie*,³ but its full consequences appear only several years later.⁴ Differences of fusion are, for Stumpf, not qualitative but quantitative. Since fusion is the unitariness of a complex, degrees of fusion are degrees of unity. Among auditory fusions, the octave is the 'highest,' the sevenths among the 'lowest.' It must, however, be borne in mind—for the sake of what follows—that Stumpf, although he

¹ *Tonpsychologie*, i (1883), 101; ii (1890), 65, 127 f.

² Ebbinghaus (*op. cit.*) brings Stumpf's fusion under one of his classes of *Anschauungen*, that of *Einheit und Vielheit* (482).

³ Vol. I, 101.

⁴ *Beitraege zur Akustik und Musikwissenschaft*, 1. Heft, *Konsonanz und Dissonanz* (1898). Stumpf declares (p. 35) that the close relation of fusion and consonance had been before his mind since 1880, and that it was with the problem of consonance in view that he had entered so circumstantially into the phenomena of fusion.

identifies fusion and unitariness, has in mind, not unitariness in general, but only that phase of it which is derived directly from sensation qualities.

O. Kuelpe's conception of fusion is closely related to that of Stumpf. Kuelpe, however, while he accepts Stumpf's account in general, gives the term a somewhat wider interpretation. He carries it from the special psychology of tone to general psychology. Fusion thus becomes one of the typical modes of connection among mental elements. It stands co-ordinate with 'colligation' (*Verknuepfung*), from which it is distinguished in two ways.¹ In the first place, "it is characteristic of the fusion that the elements contained in it are more difficult of analysis, of the colligation that they are easier of analysis, in connection." In the second place, fusion is a qualitative, colligation a spatial and temporal, form of connection: "if the connected elements are temporally and spatially identical, but differ in quality, their connection must be termed fusion; if they differ in duration or extension, colligation." As thus defined, fusion covers not only auditory complexes but also the mixture of color-tone and brightness, as well as emotions and impulses. Kuelpe's unwillingness to make degree of fusion independent of intensity, and of the number and pitch difference of constituent tones, shows that his fusion is not identical with Stumpf's consonance.²

In Wundt's psychology, fusion occupies a prominent place. It stands with assimilation and complication as one of the three types of 'simultaneous association.' It is of two kinds: intensive (between such homogeneous elements as tones), and extensive (the blending of visual, pressure, muscular and tendinous sensations in the consciousness of space). The common attribute of all fusions is the prominence of some one element. A typical case is the simple clang or note, with its prominent fundamental and faint overtones. When Wundt says that a fusion is dominated by a single element he means that this element stands in the focus of attention; *i. e.*, that it is apperceived while the remaining elements remain obscure.³ His

¹ *Outlines of Psychology* (1895), 276 f.

² *Ibid.*, 288.

³ *Grundzuege der physiologischen Psychologie*, 5th ed. (1902), ii, 110 f, 372.

fusion is thus a function of apperception, whereas Stumpf's is a matter of sensation.

The conception of fusion as an undifferentiated mass has been worked out in some detail by two recent writers: H. Cornelius of Munich, and E. Buch, of Copenhagen. Cornelius says of fusion, "whenever . . . a sum of part-sensations must be assumed, without being individually noticed, we shall . . . speak of the fusion of the part-sensations whether these stand together temporally or in succession."¹ Buch has practically the same notion.² There is fusion, he says, 'where a plurality of stimuli are correlated with a single idea'; that is to say, where no single stimulus gives rise to its own appropriate conscious process, but where every one contributes, nevertheless, to the 'fusion-mass.' These definitions substitute psychophysical terms for the mathematical and metaphysical terms of Herbart.

Muensterberg³ denies any special affinity in fusion; for affinity he substitutes inhibition of a greater or lesser number of the primitive elements (*Urelemente*) which compose the sensation.

II.

We have, in our historical sketch, sufficiently illustrated the wide ambiguity of the word 'fusion.' So great ambiguity in a technical term undoubtedly works mischief; it is not, however, in the present case, easy of remedy. Any attempt to clear it up is likely to be met by the objection that a new definition will of necessity be couched in the terminology of some particular system and cannot, therefore, be made acceptable to the science in general. I shall, nevertheless, propose a use of the word that is slightly different from any we have considered. This will be done both to bring out the common features in the various interpretations of fusion, and to suggest what seems to me to be the most profitable systematic employment of the term.

¹ *Psychologie als Erfahrungswissenschaft* (1897), 133; *Vjs. für wiss. Philos.*, xvi (1892), 404; xvii (1893), 30.

² *Philos. Stud.*, xv (1899-1900), I, 183.

³ *Grundzuege der Psychologie*, i (1900), 374 f.

In a review¹ of the recent literature on 'mental arrangement,' the word 'incorporation' was suggested for those simple conscious experiences which stand nearest the analytic elements of mind. The article was a defense and an interpretation of mental analysis into elements; and, at the same time, it was an attempt to set the final results of analysis into relation with the living tissue of mind. In another connection,² I have suggested a classification of simple incorporations into 'qualitative,' 'extensive' and 'temporal.' It is the first of these three classes—the qualitative incorporation—that is now to be brought into relation with fusion.

The peculiarity of all incorporations is, first, their unitariness, their organization; and, secondly, the presence of unique characteristics which are not to be found in the incorporated elements. The specific mark of the qualitative incorporation is the direct apposition of qualities without the introduction of spatial or temporal connectives; the members are 'given together' in consciousness, and are to be distinguished only by qualitative diversity.

Now the richest variety of simple qualitative incorporations is to be found in the auditory material which Stumpf has examined in his psychology of tone. Tonal complexes display both of the marks of the incorporation,—unitariness and special attributes,—and they give us, also, the greatest number of typical qualitative connections that are to be found in any single department of mind. Nevertheless, we cannot make qualitative incorporation synonymous with fusion, if fusion is to mean consonance;³ for consonance is only one of several moments that contribute to the unity of the tonal complex. One and the same consonance,—*e. g.*, the consonance of the fifth—may display now more, now less, unitariness, depending on attention, on intensity, on practice, on the adhesion of its members to associated processes, and on other special and general

¹*Amer. Jour. of Psy.*, XIII (1902), 269.

²An article on the psychological meaning of 'clearness' to appear in the current volume of *Mind*.

³By no means all musical theorists and psychologists agree with Stumpf that consonance is the sensuous relation of pitch-qualities. Wundt, *e. g.* (*Grundzuege*, 5th ed., ii, 421), has an entirely different conception of consonance.

factors. It is true that the sensation qualities determine the type of the incorporation; but it is not true that the unity of the complex depends solely upon these qualities. This fact assumes importance when we try to determine the *degree* of incorporation. The degree of incorporation of the fifth may be regarded as a constant quantity only on condition that it depends solely on the consonant relation of the constituent tones. So far as Stumpf means by fusion consonance,—and not unity in general, as he sometimes seems to mean,—he is consistent in ascribing to the fifth an unalterable degree of fusion that places it between the octave and the fourth. Even Kuelpe, who, as we have noted, is strongly influenced by Stumpf's doctrine, hesitates, at this point, to adopt so radical a position. No one who has introspectively lived through auditory incorporations can doubt that, under constant conditions, the unity of the fifth is greater with unlike than with like intensities, greater in half-attention than in absorbed attention, greater with like than with unlike spatial localization, greater without than with visual or articulatory associations.

If we cannot identify incorporation with a fusion which means consonance, neither can we identify it with the fusion of Cornelius and Ebbinghaus which is the converse of analysis.¹ A 'fused' mass without parts, cannot, with propriety, be called an incorporation; for the latter demands individual members. An incorporation without parts is paradoxical.

But neither can we make Wundt's fusion synonymous with this type of incorporation; both because it covers space (which differs fundamentally from qualitative connections), and because it demands a dominating element.² There is, indeed, a variety of qualitative incorporations which is marked by the predominance of a single member,—the musical note is a good example,—but it is only a variety. It depends upon some accident (*e. g.*, great intensity) which attracts the attention to a certain part of the incorporation.

¹ Cornelius' fusion also covers complexes with temporally distinct parts, which would come under our temporal incorporations.

² This demand is derived, of course, from Wundt's close union of fusion and apperception.

Wundt's fusion is too much a matter of attention; it ignores the influence of sensation quality. Stumpf's fusion is too much a matter of sensation quality; it ignores the influence of attention, of intensity and of other factors in incorporation.

Now a synthesis of these two extreme accounts of fusion would give us precisely the essential features of the qualitative incorporation. It would take into consideration all the factors which contribute to unitariness, and it would take account of all the characteristics of the fused complex.

Such a synthesis must, however, recognize a striking difference between quality and attention as moments of incorporation. We have seen that both quality and attention affect the degree of incorporation. Certain qualities are clearer than others in combination; *e. g.*, the temperature and gustatory sensations in the taste of ice cream are clearer than the olfactory and gustatory sensations in the 'taste' of wine. On the other hand, *all* qualities are clearer in attention than in inattention. But there is a difference. The obscurity that depends upon quality is *dependence*; the obscurity that arises from inattention is cloudiness, indefiniteness, dullness, confusion. A *c* heard out from an octave incorporation is neither cloudy nor indefinite nor dull nor confused; it is dependent, attached, adulterated, not properly itself, and no amount of concentration upon it can deliver it from its bondage, although concentration will remove every trace of blur and confusion.

The point, then, is this, both kinds of clearness exert an influence upon the unity of a complex. There is a unity of the complex partially analyzed by attention, and there is a unity of the sensation qualities simply as given together. Moreover, unitariness arising from all other factors of synthesis—intensity, association, habituation, practice—may be reduced to one or other of these two kinds, either to qualitative dependence or to the confusion of an imperfect attention.

Now since our qualitative incorporation embodies the important features of fusion, as fusion has been interpreted by psychologists of various schools, an identification of the two concepts may, without difficulty, be effected. The identification is proposed because it relieves the term 'fusion' from its unfortunate ambiguity while it does not sacrifice—but rather

combines—the important qualities already associated with the word. Let us see what factors fusion, as qualitative incorporation, preserves to us from the various accounts examined. From the Herbartian doctrine we rescue out of the wreck of metaphysical and mathematical theory, the fact of the closer and more remote union of 'ideas;' from Stumpf we derive a mass of empirical data regarding the structure of sensation complexes; Kuelpe gives us a systematic setting of the facts, and Wundt acquaints us with the enormous influence of attention upon the synthesis of mental formations. These are all indispensable data for a complete doctrine of fusion.

The synonymous use of 'fusion' and 'qualitative incorporation' may seem, at first sight, to render unnecessary the preservation of both terms. Nevertheless, the two words should, in my opinion, be retained. If 'fusion' be elevated above its local limitations and purified of inconsistencies, there is no reason why it should be abandoned. Indeed, quite apart from intrinsic reasons, its own inertia will tend to keep it in the literature of the science. 'Qualitative incorporation,' on the other hand, as a representative form of mental synthesis, has a systematic use in the psychology of ideational complexes which it cannot delegate to the more historic term.

THE GENETIC FUNCTION OF MOVEMENT AND ORGANIC SENSATIONS FOR SOCIAL CONSCIOUSNESS.

By Professor MARGARET FLOY WASHBURN, Vassar College.

A useful psychological distinction, though one that has not received much attention so far, is that between a genetic element and a concrete element. By the latter, we mean a process discoverable in our present mental life by introspection, and incapable of further analysis by the method that discovered it; for instance, the sensation red. The simplicity possessed by a process of this type, as it does not necessarily correlate itself with simplicity of underlying physiological process, is also quite probably not the descendant of equal simplicity in the past of mental development. Such a process, simple as it is from the very outset of the individual life, may be in its mental origin far back in the past of the species a fusion of elements now undiscoverable by direct introspective analysis, yet in some cases to be inferred on other grounds, such as for instance the known history of a sense organ. Elements of this historic significance, primitive ingredients at an earlier stage of mental phylogenesis, may be termed genetic elements; a familiar instance would be the Spencerian 'nervous shock,' interpreted as the psychic aspect of a nervous shock. As regards their qualitative character, it is evident that in some cases genetic elements may, indeed must, have been entirely unlike anything now experienced as a concrete element. In other cases, the genetic elements that long ago became indistinguishably blended into a process not now analyzable, may have been of a quality not different from concrete elements known at this present stage of mental life.

Now the importance of taking account of the concept of genetic elements becomes apparent whenever we attempt to trace the development of any process in the individual mind. It is impossible, assuming only the mental structures discover-

able by our present introspection, to give a continuous and coherent explanation of individual mental growth. There are breaks; the effect is more than the causes; the whole is greater than the structural parts we thought went to compose it. A striking example is to be found in the problem of the rise of social consciousness in the individual. By social consciousness, it is generally agreed, is meant 'ejective' consciousness, the reference of a certain mental process to another mind. Clear, fully realized social consciousness is a late product both in individual development and in the history of the animal mind; its existence in the lower animals is more than doubtful, and its defects in the human child are responsible for the cruelty often displayed by children. The most familiar attempt to explain its rise in the individual child is Professor Baldwin's appeal to imitation. The child, he tells us, early becomes interested in the movements of the persons around him, as possessing much pleasure-pain importance in his life. This interest in and attention to the movements of others leads by virtue of an inborn connection between visual and motor centers to imitation of these movements, whereby the child gets certain experiences. It is thus enabled to interpret the movements it watches, to realize their inner aspect, and to get some consciousness of the mental life of others along with the development of its own. This account of the process of awakening in the social consciousness seems fundamentally probable, but equally evident is the fact that it describes an awakening, not a construction. Given a tendency to project certain mental states into other minds, to refer them outward in an ejective as well as an objective sense, then imitation of movement offers an opportunity; but if you make the child's inference from its own experience to that of others an explanation of the tendency, you are evidently assuming the thing to be explained. How does the child come to have any power at all of thinking of experience as belonging to other minds? Nothing that we can find in our own conscious life at the present time will bridge the gap. We can only say that it is a part of the child's inherited mental constitution to give, when furnished the proper clues, a social interpretation to certain aspects of its experience. Imitation is the only congenital factor here that Professor

Baldwin discusses as such, but the tendency to 'ejectify,' if the word may be pardoned, is equally necessary as an original postulate.

Here, then, is a case where no introspective analysis can discover the element that combines to form a new mental product, the idea of an idea in another mind. I can find marks enough to distinguish the processes which I think of as belonging to my own conscious experience and those which I refer to the conscious life of another. If I take a friend to see a view familiar to myself, I have, as I watch him gazing, an idea of the impression it makes on him, which is obviously distinguished from, though similar to, the impression it makes on me, because it is associated with my perception of the movements of facial expression and gesture by which he expresses his state of mind—visual elements that are not connected with my own enjoyment of the scene. But this does not explain how I came, in the beginning, to give a social interpretation to such movements. I learned by imitation and association with language, what *particular* social interpretation to give them; to understand some of them as expressive of pleasure, others of dislike and so on; but no combination of elements now introspectively discoverable accounts for my giving them social interpretation *überhaupt*.

Is it possible, by investigating the conditions under which the higher forms of animal life developed, to find genetic elements that will meet the requirements of the case? Let us suppose an animal able to form a representation of a mental state, say of alarm, as existing in the consciousness of another animal. Observations on social animals, for instance the Medlicott pigs, indicate that the effect of a particular cry, resulting from alarm in the consciousness of one member of the herd, is to frighten the others, that is, to produce in them a state similar to that in the mind of the vociferating beast. Here there is not necessarily any social consciousness at all. Suppose, however, an animal capable not only of being scared at a certain cry, but of thinking of the author of the cry as scared. It is unnecessary to make any conjecture as to the precise epoch in development when this stage is reached, or whether any animals below man attain it. It is reached somewhere below the

level of human intelligence as we now know it, for we have seen that the ability to form ejective ideas is innate in the human infant. Now the thought of another creature as alarmed may be called an idea or representation of alarm, differing, that is, from the actual emotion as experienced by oneself at the present moment. Into the nature of this difference, again, we need not go; it is with another difference that we are concerned. An animal capable of forming such a 'free idea,' ejectively referred, is also capable of forming an idea or representation of a similar state formerly experienced by itself. How do these two representations come to be distinguished for consciousness? In other words, the mental states expressed by the two remarks, 'How frightened I was!' and 'How frightened he is!' are alike in being representations of alarm and not the actual, present emotion. How did they come to be differentiated? We are not implying, of course, that our animal has any personality ideas such as the use of 'I' and 'he' would involve.

It is a trustworthy principle, in view of the eminently practical conditions that have presided over the whole process of life-development, to assume that whatever is more essential to welfare and survival will make its appearance earlier than that which is less essential. Biological necessities will, generally speaking, precede biological luxuries. And on this principle we are safe in assuming that *certain motor reactions* of coming to the rescue, joining in defense, and so on were developed in response to the cries of a fellow creature long before any sympathetic or social consciousness of that creature's suffering was possible. We know that definite motor response to the voice of the parent animal is innate in a large number of species. Young birds not out of the egg will cease piping if the mother bird gives the alarm note. It is probable that the parent animal also responds by certain innate reactions to cries of alarm or pain from the young. In social animals such reactions are not confined to parents and offspring; a certain cry produced on the part of the wild pigs "a rush of all the fighting members to the spot." Social animals are usually dependent for preservation upon concerted defense. It would therefore be necessary to the life of such a species that certain cries

should stir, when heard, a definite impulse to seek the source of them and fight. The instinct would be so essential, so life-saving to the species, that it bears all the marks of a 'primary' or pure natural selection instinct. How blind, how far from involving any social consciousness, it is, we find well illustrated in Mr. W. H. Hudson's account of the behavior of cattle when one of their number gets into difficulties other than combat—such as being caught in the rocks. They attack the unfortunate with the utmost fury and gore or trample him to death. Mr. Hudson's explanation is that this useless behavior is an illusion of the rescuing instinct; the cries of the animal in distress stir up the fighting impulse in other members of the herd, because usually such cries occur when the animal is attacked by an enemy. There being no enemy in this exceptional case, the vengeance that should be wreaked on the aggressor falls upon the victim. "When the individuals of a herd or family are excited to a sudden deadly rage by the distressed cries of one of their fellows, or by the sight of its bleeding wounds and the smell of its blood, or when they see it frantically struggling on the ground, or in the cleft of a tree or rock, as if in the clutches of a powerful enemy, they do not turn on it to kill but to rescue it."¹

Mr. Hudson himself is inclined to think, for reasons he does not specify, that the rescuing instinct arose not through natural selection alone but "through an intelligent habit becoming fixed and hereditary." However this may be, we can be tolerably sure that the 'intelligence' originally involved did not comprise any clear ejective consciousness of the other animal's suffering; and one reason is that stated at the beginning of the last paragraph. The motor reaction would be essential to the life of the species; sympathetic consciousness would not be essential. It is of the utmost practical importance that one animal should be stirred to helping activity by the cries of another; that it should form a representation of the other animal's suffering is rather the reverse of necessary; as an unpleasant conscious state, such a representation is more of a disadvantage than a benefit. That such representations ultimately came to be formed was not because they were the best way of secur-

¹ The Naturalist in La Plata, 3rd edition, 342.

ing helpful action; they were a by-product of the growth of representative power, the formation of 'free ideas' in general. Our human sentimental acquaintances with vivid sympathetic consciousness and languid practical philanthropy suggest forcibly enough that a natural selection instinct might be practically more valuable than ejective ideas. If another reason were necessary for the belief that social motor reactions preceded social consciousness, it would be found in the fact that such reactions occur in animals comparatively low down in the scale, while recent work in animal psychology seems to indicate that the power to form 'free ideas' is very limited even in the highest animals.

But if all this be true, we have found the primitive marks of distinction we were looking for between ejective ideas and other ideas of similar content. It is through the social action stimulated by the behavior of others that conscious creatures have been led to social interpretation of that behavior. Let us go back to our animal capable of forming representations on the one hand of its own past alarm, suggested, perhaps, by revisiting the scene of it; and on the other hand, of another animal's alarm suggested by the sound of cries. The whole motor attitude is different in the two cases. These two ideas, necessarily similar in their internal constitution, would differ in the escort of organic and movement sensations accompanying them. In the first case, we should have such sensations centrally or peripherally excited as are the ordinary ingredients of the emotion. In the second, there are, besides, the sensations resulting from the stirring of an innate impulse to certain movements whose outcome is usually the defence or assistance of the animal in difficulty. From the dawn of the power to form ideas, the consciousness produced by manifestations of mental processes in another animal would contain different elements from those going to make up other representative consciousness; and these elements, the genetic elements of which we were in search, are the movement and organic sensations produced by motor reactions of social utility, already on the field before social consciousness develops.

THE STATUS OF THE SUBCONSCIOUS.

By Professor JOSEPH JASTROW, University of Wisconsin.

The interpretation to be given to that region of psychological activity for which the 'subconscious' appears to be the most appropriate designation will influence fundamentally and comprehensively the conception of consciousness, the function of mind, and, indirectly, the scope and method of psychology. Definitions of psychology have at times attempted to include this elusive portion of the psychological domain either by distinct enactment or by implication, and have at times ignored or put aside as irregular or unexplored this darkest region ever tempting to the adventurous psychologist.

A varied accumulation of material—most of it gathered in recent years—and a renewed and somewhat encyclopedic interest in the completeness of description of our psychological fauna and flora have combined to draw attention from diverse directions of approach to the intrinsic importance of subconscious activities in the functional life of the mind, and of the formative importance of the conception of the subconscious in the shaping of working hypotheses in contemporary psychology. It is quite out of the question any longer to refer to these facts in a footnote, or to treat the issues involved as merely subsidiary; on the contrary, there is hardly a chapter in psychology that can be considered to be adequately portrayed or even truthfully sketched, that fails to incorporate the significant aspects of its subject derived from the study of the subconscious forms of the processes concerned. Memory, attention, habit, association, suggestion, imitation, and the rest of the familiar list of psychological activities are to be interpreted with equal reference to the shadows as well as to the high lights of the complex reality.

The problem presents a descriptive and an analytic phase, the former concerned with a natural history of the genera and species of the subconscious, the latter with the discovery of the

principles and generalizations that unify and illuminate the data and contribute to the establishment of fundamental positions in regard to what the mind is and does. For the present survey the logical requirements of these portions of the topic, though they should be held in mind, need not dominate the order of sequence or importance of the arguments to be advanced. It is mainly with the interpretation of the data together with an indication of their varieties and significance that we shall here be concerned.

The "subconscious" in turn presents two aspects, partially suggested by the substantive and by the adjective use of the term. The latter suggests that an activity which might be and usually is presented in conscious recognizability, is reduced or submerged to a sub-threshold degree; the former gives a hint that these submerged or outlying activities themselves may organize and co-operate and form an aggregate, itself an important number of the *dramatis personæ* of the psychological cast. In what sense, then, do subconscious activities exist? What is the psychological status of the "subconscious?"

Historically as well as logically the problem begins with subconscious sensations or the stimuli necessary to arouse them and leads to the formulation of the concept of a sensation-threshold. I do not hear the lapping of a single wave against the shore, but the accumulation of just such waves produces the roar of the sea. If I hold to my nose a single wood violet I can detect no odor; but a bouquet of these leaves a distinct impression. This is the absolute threshold. Still more significant for the mental life is the relative (or differential) threshold; the formulation of which summarizes a considerable array of evidence that when differences gradually decrease they fall into the region of the psychologically imperceptible, though the physical differences of the stimuli concerned may readily be established by simple physical tests. Two bowls of water seem equally warm to the finger, though not to a sensitive thermometer; two weighted boxes seem equally heavy, though so crude a physical apparatus as a grocer's scale at once indicates which is the heavier. Or to make the experiment more precise, the difference between 100 grammes and

102 grammes is an "imperceptible" difference, but five times that "imperceptible" difference, or the contrast between 100 grammes and 110 grammes, is a "perceptible" difference. What is the basis of this distinction? First it is obvious enough that there is no sudden drop, no "jumping-off place" in the transition from perceptibility to imperceptibility; and this is true both of the absolute and of the relative threshold. Parenthetically, it may be observed that the distinction between these two types of threshold, though important for practical purposes, in large measure falls away so far as the interpretation of the psychophysical relations is concerned; the absolute threshold becomes an expression for the capacity of a stimulus to raise itself up above the general murmur of diverse sense-stimuli to the clearness of a separate hearing, the relative threshold an expression for a like and individual differentiation in kind or degree from among the similarly arrayed candidates for mental notice.

The psychophysical process, the correlative action in the nervous system that accompanies the existence of the imperceptible difference, is very probably not intrinsically different in kind but varies only in degree from that which gives rise to a perceptible difference. Accordingly it is entirely natural that variations of condition will determine the perceptibility of a sense-impression. A difference—such as that between the heaviness of two weights, the brightness of two lights, the quality of two tones—is perceptible if the two impressions are presented in immediate successive contrast; allow a few moments to elapse between the two sense-impressions, and the perceptibility becomes uncertain or disappears. Still further it has been shown¹ that if in the presence of such imperceptible or sub-threshold differences, one persists in making judgments, which are wholly without confidence—seem, indeed, mere guesswork, without any conscious appreciation of that "local sign" which, if sufficiently magnified, would serve as the ground of their differentiation—the percentage of correct judgments will be larger than mere guesswork would produce; and the percentage

¹ First, I think, in the paper by Mr. C. S. Peirce and myself "Small Differences of Sensation" (Proceedings of the National Academy of Sciences, 1887).

of success will be greater for differences of stimuli but slightly below the (conscious) threshold value than for differences considerably below that level. A very interesting and wholly different bit of evidence for the similar influence of the imperceptible is furnished by a recent experimenter¹ who has demonstrated that in the case of the well-known illusion by which a horizontal line with a *divergent* pair of oblique lines at each end (arrow tips) seems longer than a line of equal length with a *convergent* pair of such arrow tips, the illusion persists (in moderate degree and in the average of a sufficient number of comparisons) even when the arrow tips, which are formed by shadows, become so faint that to the carefully observant eye they are quite imperceptible. These instances by no means stand alone; they may be supplemented, in less quantitative form, by a large array of normal experience going to show that sense-impressions, themselves imperceptible, *contribute to and influence* the behavior of consciousness. A constant and frequent intercourse takes place between the two realms; indeed, the boundary line between them is not a natural separation, but is in large measure of our own making, a practical concession to convenience of description.

It would very probably contribute appreciably to our clearness of conception of the nature of such subconscious impression were we acquainted with the neural substrata that represent their inseparable occurrence in, and by means of, a bodily organism; we might then know whether the registration of one of these imperceptible impressions proceeds quite in the same way, though with a lesser degree of energy or sphere of influence, as one that arouses consciousness, or whether the latter sets up some kind of brain activity that is not participated in in kind or degree by the neural correlative of imperceptible impressions. But our views upon this point are not likely to be illuminated by direct evidence of this kind; the views of the neural processes will be shaped inferentially from the psychological evidence that may be brought to bear upon that aspect of the problem. The evidence—and its variety will be suggested by later considerations—seems to me wholly to support the

¹ Dunlap: "The Effect of Imperceptible Shadows on the Judgment of Distance," *Psychological Review*, Vol. VII, p. 435.

position that from the neural as well as the psychological point of view, a subconscious impression is closely affiliated to, is kith and kin with the conscious factors of experience.

The fundamental position thus reached is likely to be formulated by saying that the activity of mind, and with it the scope of psychology, is broader than the account of it obtainable from the direct perceptions of consciousness. Such formulation rejects the traditional position that "psychology is the study of states of consciousness," that its universal search-light is consciousness, and that what thus remains unrevealed is of wholly subsidiary import, to be included in a detailed inventory, but not essential to a rough blocking out of the mind's possessions or characteristics. The formulation that to me represents a truer perspective of importance reconstructs the import of the term consciousness so as to include within the vital meaning thereof these equally characteristic subconscious forms of its activity. Consciousness means not full awareness, focal introspection, but stands for the lights and shadows of the picture; its *chiaroscuro* refers to the entire distribution of distinctive forms of mental experience among the details of the situation.

It has become well recognized that the dominance of the introspective organon is supreme in the psychological world; that experiment does not oppose or restrict its testimony but far extends, deepens, clarifies and makes more precise its results. It supplies the gauging eye with a foot rule, the estimating hand with a balance; or it gives to the retina the enlargement of lenses and all the devices of increased and extended visibility, from the spectrum analysis of distant stars to the minute structure of microscopic nerve cells. Yet the introspective factor is only shifted, not eliminated. It would seem, then, that the subconscious introduces into the world of mind factors that are removed from introspective observation, and in so doing questions the distinctive trade mark of the psychological. But such is in reality not the case. The subconscious activities of the mind may be subjected to the criticism of introspective, if only we have the ingenuity and the opportunity to make them speak. Truly there is mystery enough in the inner life of the mind, but it inheres no differently in the subconscious than in the conscious operations thereof. Familiarity

has blunted our appreciation of the underlying ignorance in spite of superficial understanding of the commoner forms of mental experience; and their unusualness has emphasized the more striking and irregular instances in which quite extraordinary results must be ascribed to the operation of subconscious processes. To reinstate a proper view of the status of the subconscious in these aspects it becomes necessary to pass in review some of the most important types of its manifestations.

Our first group of illustrations was drawn from the field of sensory appreciation; its complement is that of motor expression. The relation of the subconscious to the subvoluntary is interesting and vital; the semi-observed and the semi-intentioned are as real and typical factors of conduct as are the reactions to which we purposely give heed and deliberately determine and execute. For both participants, as for the bond of relation between them, we formulate the concept of the automatic and the sphere of habit. I go all through my papers looking for a check received in the day's mail and find it already in my wallet, yet have no recollection of putting it there; I catch myself reaching for my watch over to my left waistcoat pocket and in so doing recall that in these summer days it is carried in a special pocket of my trousers; or I raise my hand to my head to lift my hat in greeting a lady, and try awkwardly to correct the movement initiated as a hat-raising one into the very different manipulation of a soft cap. These are merely the striking, because perchance misplaced, issues of subconscious automatisms. Just as illusions bring to light, in more striking form, the same subconscious types of sensory judgment and appreciation that are utilized a hundred fold more frequently in normal and commonplace, correctly interpreted experience—so, too, lapses of speech and conduct set forth more patently but not more typically the underlying structure of subvoluntary performances. The secret of the more extreme and unusual cases is to be sought in the rationale of the usual and commonplace; a common key will unlock the various compartments of the subconscious life.

While this view of the matter may not explain, it at all events vigorously opposes types of proposed explanations inconsistent therewith and places the source of the difficulty where it be-

longs; it is an aid to diagnosis if not to treatment. It tells us that if only we could explain memory—the fact that impressions now made, to-morrow disappear and weeks after reappear—if we could explain what is a disposition, a sensory or motor habit, which makes my expectation anticipate and falsify reality and makes me do more easily, more naturally and with less effort, what at first I do with difficulty; if we could satisfactorily account for the *provenance* and the manner of coinage of these pennies of the intellectual medium of exchange, the pounds would take care of themselves.

Psychology owes a great debt to Professor Lloyd Morgan for his masterly presentation of the spread of consciousness with the vital differentiation of its focal and marginal elements. The transition from marginal to focal, the influencing of focal factors by elements that persistently remain marginal, the ever widening and fading penumbra of the marginal field—these indicate an entire consistency with the view that the conscious includes the subconscious, that both participate with distinctive functions in a common form of mental action. Psychology is as intimately concerned with the marginal and with the most outlying portions of the marginal as with the focal; its attitude and mode of explanation are no different for the one than for the other. The question, however, still lies open as to whether outside of the entire conscious field there lies a province under other sovereignty, removed from a central control, not a subject of consciousness at all. This is the question more properly discussed as that of the unconscious; neither with the philosophical nor with the psychological status of this problem is it my intention to deal; its mention is necessary only to prevent a possible confusion between the two (closely related) issues. When Dr. Carpenter (about 1850) stirred up a controversy as to the existence and nature of “unconscious cerebration,” he and his opponents were for the most part considering the nature of subconscious activities. The controversy was in part crude and the issues much confused; but the outcome was a distinctly more general recognition of the large share of influence belonging to mental activities that do not normally (though they in part may be made to) appear in consciousness.

A logically consistent view of the unconscious would posit the existence of organized groups of apperceptions that for the most part live a life of their own ; or parasitically taking their nurture from the host upon which they have fixed themselves, yet zoölogically and functionally remain quite unrelated to the structure and activities of the chief partner of the organism. Those who have the courage to hold this view in its psychological form seem to have been driven to it by the more abnormal manifestations of the subconscious activities. They have been impressed with the fact that certain persons can write automatically with the one hand while the other may be very differently occupied ; and the message thus resulting come with all the surprise of novelty and extraneous origin to the writer's active consciousness. Though connected with the same brain the right hand seems not to know what the left hand is doing ; just as in other instances the subject, when normally conscious, knows nothing of what he (more typically she) did when hypnotised, and, when hypnotised, reveals knowledge of data which the normal conscious volition cannot command. Still more strikingly, the experiences thus hidden from the normal consciousness may actually replace or alternate with those of the normal personality and give rise to those sudden mutations of personality, hypnotic assumptions of various rôles, distinct cycles in the epos of a "spiritualistically" controlled or entranced medium, or the quite regular alternations in mood, manner and memory-possession in the hysterical. It is in this field that we meet with hypotheses and terminology to set them forth, from the crude recognition of this partner ego as in control of the other cerebral hemisphere to the equally baseless assumption of an "objective" and a "subjective" ego ; and, intermediately, with subliminal selves, submerged strata of consciousness, "unvisited psychological lumber rooms," split-off personalities, and the like,—a "tumbling-ground for whimsies," surely, as Professor James cautions us that speculations in regard to the unconscious are apt to produce. Now, so far as we have any insight into the nature of these phenomena, it becomes clear that they in no wise sanction the hypothesis of a separate subconscious organization. Two things are clear above all: first, that intermediate between these extreme ex-

amples and the commonplace incidents of the life of the subconscious, are many forms of manifestation forming a bridge of analogy from the one to the other. Dreaming is in itself a sufficiently versatile process to manifest them all. For in dreams "a number of different personalities occupy the stage at the same time, each representing a different point of view, each ignorant of the next move of his fellows, and yet there is nothing strictly unconscious nor any absolute cleft in consciousness, for all the *dramatis personæ* are included in the larger single mind which is their theater."¹ In hypnosis as in trance-states, in hysteria as in spontaneous alterations of personality, there is abundant evidence that the subconscious is then in close communication with the conscious, and that suggestion has often furnished the key by which the passage from one to the other may be opened up. Read in this light such a story as that of Professor Flournoy's Helène Smith becomes intelligible; posit an independent realm of consciousness (call it the subconscious or the unconscious) and you have chaos or "a tumbling-ground for whimsies." The issue reminds one of the little barometric contrivances in which one fantastically arrayed figure comes forward in fair weather and another on dark days; the fair-weather-consciousness knows nothing of the other, and the low barometer acquainted only with cloudy skies would find incomprehensible the optimistic temperament of its ignored partner, if, indeed, it could be made aware of it at all. As a fact, however, there are not within us two souls with not a single thought in common, nor even one heart that

¹I take this sentence from Professor Stratton's chapters on the unconscious in his recently published volume (*Experimental Psychology and Culture*, 1903) and desire to use this opportunity of recording my satisfaction in discovering that his conclusions are so closely parallel to my own. So close is the agreement as to render it pertinent for me to add that my own discussion was well formulated and partly presented verbally before a meeting of psychologists in Chicago in November, 1902, and again in Washington in December, 1902, before I was at all aware of Professor Stratton's contributions. In the writing of the present essay, I have profited much by his chapters, and I have allowed his presentation to stand for certain aspects of the problem which I had intended to treat, but in which he has adequately anticipated my own position.

beats as two. Abnormal as these classifications of co-ordinated experiences surely are, their explanation does not justify the hypothesis of a separate subconscious mind. They cannot overthrow but must be assimilated to the vast aggregate of normal evidence for the intrinsic kinship of the conscious and the subconscious. Not only, then, do the abnormal data not demand or justify the hypothesis of a separate, independently organized "subconscious," but such an hypothesis actually devitalizes and obscures the significance (so far as understood) of these phenomena. In the second place, to resume the enumeration above indicated, every hypothesis of this type should be broad enough to include both the normal and the abnormal phases of mental action, and should more particularly take its clue and reveal its validity through its power to illuminate and comprehend the normal mental life. The hypothesis of the independent subconscious—the pale shadow of the flesh and blood partner, as the cover-design of a well-known book on the subconscious visualizes it—sins against all of these requirements.

I am well aware that this eclectic presentation of a discussion provoked by a recent survey of the facts and of the literature concerning subconscious activities—a survey that will in due season lead to a more systematic publication—does not constitute an adequate brief in behalf of the extension of domain of the normal conceptions of consciousness over the subconscious. Yet in view of the many hypotheses, and of treatises presenting them, that argue for the opposite view—and the extreme forms of which, however slight a hold they may have upon the professional psychologist, yet decidedly influence the lay reader and the interested public—it seems distinctly worth while to reinforce the position, no doubt more or less indefinitely approved by many, by more explicit statement.

The subconscious activities may accordingly be brought to light (1) by direct evidence, that intensive concentrated consciousness fails to reveal the causes in operation that do none the less contribute to and influence thought and action. In the simplest form of such influence we find that sensory stimuli which with the most intensive attention of which we are capable seem to register no effect, yet can be experimentally shown to be capable

of influencing our apperceptive processes. This is but one sample of this form of evidence; others are contributed by the formation of habit, by unconscious inferential processes in normal sensation, by sudden budding forth of memory images and the like. Certain of these instances are intermediate in form and lead to (2) the extensive spread of consciousness in which the marginal elements constitute the subconscious factors; such factors may in large part be brought into the focus by direct conscious effort. Such effort is variously successful in the introspectively expert as contrasted with the novice, according to the degree to which the fixity of interpretation has crystalized into a habit difficult to overthrow, and in the end according to individual differences of mental constitution. Yet the recoverability of much of it—the possibility of shifting the search light of consciousness over a considerable area—has been sufficiently established to justify the extension of such conceptions to the field of the subconscious as a whole. Both intensively and extensively, the subconscious thus establishes its kinship to, its right to a seat at the hearth of consciousness. The corollaries from this position are many. They extend to the abnormal as well as the normal; they appear best in a descriptive account of the varieties of subconscious activities in their functional rôles in actual life; they distinctly favor the unity of structure of the mental organism and as determinately oppose the hypothesis of a vital subdivision or partition of such organized activities. While the strength of the view thus advanced depends upon the descriptive evidence, which it is my intention to set forth elsewhere, it seems to me that, from the analytical point of view, a result both consistent with and corroborative of the inductive résumé can be established. Such establishment, and a more universal recognition of the functional import of the subconscious in the general mental life, will go far to reinstate to its proper importance the status of the subconscious in contemporary psychology.

AN ATTEMPT AT ANALYSIS OF THE NEUROTIC CONSTITUTION.

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When considering the etiology of mental disorders we should distinguish the cases in which a person in the height of health and development is taken by a more or less definite illness with mental disturbances, from the cases in which a lingering condition of constitutional or secondary weakness is aggravated by a certain disease.

It is, consequently, desirable to start with a few statements concerning the constitutional defects and chronic subacute and acute states of debility, such as may usher in one of the more definite disease-forms.

Here we meet at once the favorite term of "run-down condition," unfortunately as vague as its therapeutic counterpart, the "tonic" and, let us hope, not an insurmountable difficulty but chiefly a cover for defective determination to make accurate examinations, and a consequence of the exclusive, and perhaps wholesome, interest of modern pathology in specialities which yield more glory with easier and more conclusive work.

A step towards discrimination has long been made by the creation of the concept of *diathesis*, and its broader foundation, the "constitution." Discarded for a long time these matters are being brought back to the notice of the physician by the introduction of more trustworthy methods of study. During a fairly broad course of medicine I never had heard the topic spoken of in the later 80's of the last century, except in allusions to the *habitus phthisicus* and the like, and was really surprised to hear it made the subject of a series of lectures in the course on the practice of medicine by Sir Grainger Stewart in Edinburgh, 1890. He enumerated the classical constitutions and diatheses:

1. The nervous constitution: generally with fair complexion, bright eyes, frequent change of color and facial expression, the

bones and muscles not vigorous; the heart, like the nerves, excitable.

2. The lymphatic constitution: with great head, irregular fleshy face, slow weak pulse, large hands and feet, etc.

3. The sanguinous constitution (Scandinavian race): fair hair, blue or gray eyes, easily flushing face, strong and excitable heart, but no nervousness.

4. The bilious constitution: with a tendency to obesity, dyspepsia, diarrhoea, etc., and melancholia.

Further the gouty, rheumatic, strumous, and syphilitic constitution, etc.

A certain practical justification of such a classification is quite undeniable, and attempts are slowly coming up again in the form especially of two types of study:

1. The individual psychology.

2. The types of functional efficiency, or insufficiency, such as are being established by Kraus and Martius for the heart and stomach.

The problem of immunity, too, gives a few valuable allusions to the question of temporary or fundamental constitution.

In psychiatry and neurology there is especially one type of interest, the psychopathic-neurotic type. It lacks as yet sufficient definition and to analyze it will be one of the first tasks of a conscientious etiology of mental and nervous diseases. Since many individuals of this type belong to families in which a family tendency is present, it is usually dealt with peremptorily under the heading of *heredity* and hereditary statistics seem to dull the interest in a collection of accurate facts although numerous cases occur in which no heredity is demonstrable. The confusion has even been increased by the popularization of the term "degeneracy," which is used promiscuously with heredity and individual deterioration. The principle of heredity and degeneracy had, however, better not occupy us before we have made a good investigation of what abnormal constitutions we can recognize in the individuals called nervous or exposed to nervous and mental disorders.

Types of persons are difficult to define. Once for all we should give up the idea of classifying them as we classify plants. We deal with a sum of items of which each can vary; whereas

botanical classification only mentions the differential traits which would make sure that a seed of the plant would again grow into the species of plant which is thus distinguished. The issue of species is settled by the laws of heredity, while the varieties of people must be classed according to different principles. *The best medical standard is that of adequate or efficient function.* Martius has pointed out that concerning the function of the stomach we can recognize types with permanent constitutional deficiencies; Krauss has made the functional efficiency of the heart a standard for types of circulatory constitution. And in a similar way we classify people for their efficiency in those mental adaptations which we know to become actually deranged, the emotional sphere, the equilibrium of reason, or for their susceptibility to febrile delirium, alcoholic intoxication, effects of sexual excesses, etc. Further, we put forth as types of "constitutional inferiority" in the psychiatric sense certain forms of special nosological or symptomatic traits.

In the process of emancipation from traditional and untenable views of man, an iconoclastic attitude towards all attempts at practical characterology and theories of constitution was probably the only safe procedure. The existence of special types is nevertheless obvious to common sense, and when we feel the need for a practical utilization of such data, it would be wrong to deny one's self the privilege of taking them for what they are worth. The call of warning "back to morphology," or "back to what can be studied with mathematical, physical, and chemical accuracy," has its good sense and value; but since, in practical life, we know and speak of types, there is no harm in attempting to come to an agreement as to just what is to be understood by them. Physiology and psychology and anthropology have attempted it with their own specific problems; we physicians have our own, and while we deplore the lack of medically helpful material in the existing literature of individual psychology, we need not be discouraged, and shall do well to use our own methods and needs as our guides.

The purpose of characterology is to give a forecast of what a person would do in a considerable variety of emergencies. As alienists, we shall especially have to try and find out whether persons show any combinations of reactions which would make

them in our eyes candidates for mental derangement, or which would modify the form of mental derangement which they might happen to get.

Of late years the herculean task of defining characters has been taken up from several sides. Fr. Paulhan necessarily makes his classes from several points of view, just as we are forced to do for the questions of heredity. I mention his divisions because they will be of some help as an explanation of why we consider the task far from hopeless. He recognizes the plurality of lines of efficiency or defect in the same individual, because various functions are to be considered and many types of combination are possible. We can only mention the large headings of his book "Les Caractères."

Paulhan starts with the types produced by the predominance of one special form of activity. He analyzes them according to various types of association, *i. e.*, various ways in which the streams of interest and activity shape themselves. He starts from the well-balanced, and the harmoniously purposive; passes to the types in which inhibition and reflection predominate (those who are "masters of themselves"); then to a type of great interest to us, that in which associations by contrast abound, persons who inevitably think of that which is not, that which is different, that which might be, instead of acting in the healthy common-sense way on that which is before them and leaving the contrasts as a matter understood, or of value when there is a special cause for considering them. He calls these types "the uneasy," "the nervous," "the contrary." Another type, also of importance for us, is that characterized by predominance of association by contact and resemblance, that is persons in whom the inner interests are not the chief guides of their activity. What they meet accidentally while they are doing other things, becomes permanently fixed in their memory, such as conditions under which they read a book or hear some music; while they pursue something, they notice other matters and divide their attention and may even drift completely from their topic. Where this trait is predominant, the feature of distractibility is apt to influence the course of life considerably, and there results the last type from the point of view of association and characterized by an independent activity of mental

elements, the impulsive, the variable and compound, the incoherent (of as it were crumbly interests); finally the suggestible, the weak, the distracted. We might make a scale in which the individual with relatively steady plans stands at the top and is followed by those less dependent on themselves, and more and more easily influenced, until we reach those types in which the cohesion of personality is very slight, and the person is a prey to circumstances.

Another division is that according to the definite qualities of tendencies and mind, considering the breadth of personality,—the broad and the narrow; or considering the purity of the tendency,—the calm and the troubled; considering the strength,—the passionate and the enterprising and the hesitant; considering the persistence,—the energetic, obstinate and constant; and on the other hand,—the weak and changeable; from the point of view of adaptability—the pliable, the inconsiderate and the unadaptable, and from the point of view of sensitiveness,—the wide-awake and impressionable, and the cold and phlegmatic.

In the second part of the book Paulhan distinguishes the types determined by the predominance or absence of some tendencies: in the first place, those tendencies which refer to an organic appetite, the types of the high-liver and the sober; those sexually excitable or cold. Then from the point of view of mental functions: those principally visual or auditory or gustatory, or principally motor; further the intellectual, the emotional. Then he passes to the types determined by social tendencies: the egotists and altruists; and types in whom love or friendship or family affection is predominant. Then those types whose interests go mostly in the direction of communities, or of the national feelings. Then he puts together types with predominance of impersonal tendencies, the worldly, the professional; then with regard to property,—the miser, economist, the generous, and prodigal. Then the vain, the proud; those eager for fame. Then the domineering, the ambitious, the submissive and other types; and finally as compounds of these special types,—the happy, and those enjoying themselves; and on the other hand, the pessimist and those denying themselves. Moreover he speaks of tendencies which stand above the social relations, the

general idea of duty; types of political passion and of religious interest, mysticism, etc.

In the third part, he shows how these various elements co-operate in the constitution of the individual.

This brief summary may induce the reader to study Paulhan's work as an attempt to bring some order into complex facts. It is obviously our duty to develop along similar lines some definite descriptive entities for the characterization of those features which lead over to the directly odd or abnormal character.

We start from the truism that *a large number of those who become insane, are individuals in whom a turn to the worse could be anticipated*. Are the indications open to any sort of analysis? The retrospective method of analysis is the only one available now in the majority of cases. Perhaps, among intelligent and observing families, it can be pushed much further than is actually done. Moreover, when we know better what to look out for, we may undertake studies of *developing* abnormalities which are not insanity yet and follow them out so as to accumulate material of *actual observation* on which to build a solid theory of constitution.

Kraepelin in his *Psychologische Arbeiten*, Vol. I, p. 78, mentions, that it is probable that the mental constitution of the neurasthenic, the hysterical, the paranoic and the maniacal-depressive is different from the very start; but he does not tell us of the actual distinctions. He expects them from his method of biological tests. We certainly must do something to outgrow the stage at which "degeneracy" is considered a sufficient verdict instead of being shown up as a block in the way of much needed knowledge.

The development of man is not a simultaneous evolution of all the traits of the complete adult, but one function after another comes to maturity, and as a rule there is an uneven development. Nobody is perfect in every respect. The special organs which make up the human cell-colony and the uniting links of all these organs, the circulatory apparatus and the nervous system, may all demand special tests of efficiency, as has been shown by Krauss for the heart. Each apparatus may have its ups and downs, or actual defects, and even show a more or less final tendency to deterioration, either from defective endow-

ment or through defective chances in life. The biotrophic energy or vitality of *each* organ should be determined in order to arrive at a summing up of the constitution of the entire person.

It is obvious that there is no limit of time for the development of traits which would have to be laid to the individual endowment. A child that appeared normal may show failure in coping with puberty, and a perfectly well balanced and healthy person may show premature senile reduction, and we *may* find this to be a peculiarity of the family. In some children we may be able to trace the abnormal development to harmful surroundings, such as acquisition of abnormal habits, to defective nutrition in periods of growth, to a disease or traumatism; in the arteriosclerotic senile we may be able to point to alcoholism, nicotism, physical over-work, etc., in the absence of all family tendency. Hence the rule that we shall first outline the facts in the case and analyze the positive causal factors *before* we assent to a negative conclusion, such as the admission of an unknown and undemonstrable agent, as heredity, is all important. It is obvious that a really satisfactory analysis is only achieved where we can point to specific factors which caused the deviation from the normal, with something like experimental necessity. Although a large number of cases will not be open to explanation, we speak provisionally of heredity, when we see a disorder occur several times in a family. But this provisional statement is all we should imply by heredity in medical language. Moreover, where we find peculiarities of make-up we must remember that many of them must be ranked as normal and do not lead to further trouble, except, perhaps indirectly through the clashing with the environment; while other peculiarities are beginnings or agents of the undermining of the make-up and would interest us more.

In our analysis we shall now try to establish some differentiation in order to get over the extremely unsatisfactory haziness of terms like degenerate, neurotic, etc. We shall try to distinguish certain groups; but we must submit all these cases to the question: Do we deal with persons in whom some incidental affection of the brain or malnutrition during development or constitutional disturbances, like rickets, or poor educational

conditions, has produced that state of affairs which has left scars or residuals and stamps the person as one maimed in various directions by more or less different causes, and for such reasons left with an inadequate material for development and the strain of life? Or do we deal with persons in whom, with or without such residuals from early development, there are present and still in operation various vitiating influences, such as disease, anomalies of constitutional metabolism, abnormal toxic or sexual habits, an inadequate and unsatisfying life, etc.?

With this in view, we have to review first the various stages of development.

Constitutional defects from infancy are very frequent and manifold. Those defects which lead ultimately to dependence are naturally most important and best known. They are classed as idiocy and imbecility (feeble-mindedness). The best available statistics (in Switzerland) show that 1.53% of all the children between 7 and 14 belong in this category. The marked forms need not occupy us. They present a tremendous field, since imbecility includes the results of everything that can possibly leave traces in the pathology of the nervous system and mind during infancy. In all these disorders we must, of course, be prepared to see beside the functions demonstrably impaired from childhood, defective evolution of functions which should have matured later and may have been affected in the bud, and it is quite conceivable that certain peculiarities of development in later life might be due to alterations brought about in undeveloped stages, where the existing functions appeared to recover completely. This might hold for the effects of asphyxia, infantile convulsions, disorders of teething and early nutrition, traumatisms. The number in whom actual facts are demonstrable is small and apt to discourage one; but what is obtained is all the more valuable in the struggle against fatalism.

The constitutional development of the *child* is of greater importance for us. Many cases of imbecility begin to show here, but moreover a large group of poorly known and poorly differentiated types of peculiarities which, without doubt, play an important part in the abnormal constitutions of later years. Poor habits of sleep with fearful dreams, somnambulism,

emotionalism, idiosyncrasies, irritability, being startled and unbalanced easily, distractibility are often complained of. The nervous child may show from the start or gradually the more specific traits of the epileptic constitution, of the hysterical, the neurasthenic, etc., which we pass over, because they are more apt to appear later.

Puberty and adolescence are the decisive period for the formation of the make-up and for the cropping out of many defects (see Marro, *La Pubertà*, and Clouston, *Neuroses of Development*). Here we should deal with many types usually left to the pedagogue; but I refer only to the following traits: The normal youth develops an individuality with personal aims. A large number of young people remain children of the moment, distractible, swayed by desires and casual opportunities, showing flashes of enthusiasm and emotional display, but without cohesion or sound plan and consistency.

Another extreme is the prematurely and one-sidedly conscientious. In this type there is frequently a furor for abstract matters, exalted religious and moral standards in marked contrast with the actual immaturity of the conduct in the frequently precocious sexual development; periods of fantastic day-dreams and perhaps lying; an increasing isolation and aloofness from chances of wholesome correction by intercourse with the average companions of their age, combined with a keen eye for the faults of others; a clinging to older persons and isolation in matters in which the youthful instincts are deficient or abnormal, such as interest in games, and in sociability. The intercourse with older people and the great interest in words and books rather than actual experience often give these young people an apparent start as compared with the average of their age, and many parents have been children so little themselves that they overlook the danger. It is among these persons that the lack of normal balance is especially apt to lead to the appearance of overburdening, of overwork, and all those traits which mark the legion of nervous people of to-day. Irritability, outbreaks of temper, erratic and unaccountable actions break through; or the young persons become too good for the world, seclusive, fault-finding with themselves and with their brothers and sisters. They become as egotistical in actions

as they may be altruistic in words or in public. Interests in perfectly remote religious and philosophical matters do not make up for the defect in that which is most important, the adaptability to life as it is, with a healthy independence. Very often a decided change along these lines shows itself with or shortly after puberty. The connection with often quite precocious abnormal sexual practices is exceedingly frequent. They are a very aggravating factor, as they increase secretiveness, morbidly imaginative cravings and many signs of nervous exhaustibility. The danger awaiting the inconsistent easy-going is more often that towards social dissipations in alcohol and venery and their consequences; and that awaiting the exalted seclusive, the development into neurasthenic, hypochondriacal and dementia-præcox types; while hysteria, psychasthenia, and epilepsy appear on somewhat more independent ground. Various peculiarities not necessarily combined with nervousness appear, but more usually in later life.

From the general picture of nervousness we now should attempt to select and discriminate certain types and especially to define certain names and distinctions:

1. The psychasthenic. This is a term lately applied by Janet to a group of psychopathic and neurotic conditions which comprehends obsessions, impulsions, manias, phobias, scruples, tics, states of anxiety, etc. These states also figure under the term of constitutional neurasthenia, but are not necessarily connected with the truly neurasthenic complex. The ground of these disorders is a special type of character; these persons are aboulie, undecided, hesitating, timid, not combative, not able to take the world as it is, idealistic, longing for love and kindness, and, correspondingly, with ways that solicit a kindly and just attitude; they are misunderstood and meek; easily led or misled; they need stimulation and are apt to yield without decision, notwithstanding their usually superior intelligence and vivid imagination. This leads to a life given to avoiding troubles, decision and action. The child avoids active plays and is perhaps encouraged by solicitous parents; the choice of occupation is away from the trying struggle. The young man or woman shirks responsibilities, is passive in questions of marriage and choice of work. New situations, a threat, or a joke,

examinations, new religious duties, or some emotional shock prove too much, and bring forth the symptom-complexes so well described by Janet.

2. The neurasthenic, closely allied to the above. The term should be reserved for the cases combining the symptoms of great exhaustibility and irritability, depending largely on the mental attitude of lack of repose and of ready recoverability, frequent head-pressure, palpitation and uneasiness of the heart, gastric disorders, phosphaturia and oxaluria, and in men especially, often abnormality of sexual responsiveness. It is necessary to distinguish acute forms following exhaustion or infectious diseases in persons without hereditary or constitutional defect, the subacute and chronic forms or habit-neurasthenias frequently without heredity, and the chronic constitutional type, said to be to a large extent familial. It is frequently associated with the psychasthenic type. It may be well to specify cerebraesthesia, myelasthenia, gastro-intestinal neurasthenia, vaso-motor and sexual neurasthenia.

3. Frequently associated with other traits of nervousness, we meet with *hypochondriasis*, usually built on a feeling of ill-health which leads to self-observation and explanations. These are apt to become the center of thought and interest, are elaborated, or the person merely is troubled with vain fears over trifles, consults quack literature, etc. On the whole, the impressions are apt to become dominating.

4. The hysterical constitution.

Dana gives a picture of the simple hysterical constitution as consisting of crises of an emotional character and an interparoxysmal condition of emotional weakness, nervousness, hyperæsthesia and pains in the head or back, poor sleep, disagreeable dreams, globus, and vasomotor instability. These patients are mostly girls or young women, unduly sensitive, depressed, easily alarmed; they feel nervous, lack emotional control. It is rather difficult to say whether these forms are necessarily hysterical, and not better classed vaguely as nervous instability, until some characteristically hysterical symptoms occur.

Ziehen limits the hysterical constitution to emotional instability, egocentricity, craving for attention, peculiar predilections, disorders of imagination and attention (fantastic insta-

bility). He refers to sensory symptoms, regional, or referring to special objects; and to peculiar illusions and hallucinations of vision (hypnagogic or with open eyes) following emotional episodes or accompanying headache; usually with insight and without loss of memory.

To start with we must discard the popular use of the term hysterical as not sufficiently coincident with the nosological term. It is to be replaced by statements of the actual symptoms, such as emotionalism, or simulation or exaggeration, or craving of attention, which may or may not be hysterical and had best be called by their plain name. I am inclined to refer to hysteria all the mental and physical disorders which are produced by the effects of an emotion or idea which may work unconsciously to the patient, so that the simulation claimed by others is usually beyond the control of the patient and the whole explanation best accessible in hypnosis. On close investigation it usually is possible to see the foundation of the varied disorders in a peculiar limitation of the field of consciousness, and range of thought, frequently with additional exhaustibility, and the existence of emotional trauma or instability.

These disorders appear either on a broad constitutional basis—perpetuation and one-sided elaboration of traits inherited or acquired during the years of development—or they come with some other disease—hemiplegia, tabes, etc., or after some sudden shock. Many of the same symptoms may also occur in other grave constitutional or other disorders, without being plainly on hysterical ground (as it were, symptomatic hysteria).

5. The epileptic constitution manifests itself largely before or after the convulsions in signs which might be called part of the fit; Ziehen mentions, as a form of aura, hallucinations such as a threat or a stab, or the vision of a huge figure, or anxiety with precordial sensations, or the recurrence without motive of some vivid memory. In the intervals there is a certain *irritability* with occasional violent outbreaks regardless of consequences, or peculiar unwarranted sulkiness or periodic dipsomania. Later there is an increasing defect in mental capacity.

6. Of much more importance to alienists are certain types already akin to definite mental derangements. I refer to:

a: The unresistive (responding easily to fever, to intoxication).

b: The maniacal-depressive type, described by Hecker, and the constitutionally depressed—to be distinguished from the neurasthenic by the more direct feeling of insufficiency, not secondary to exhaustibility, and more likely to lead to suicide, and by the occurrence of periods of elation.

c: The paranoiac type, continually ready to see a meaning in things, suspicious, and at the same time with growing inclination to isolation. These persons are continually concerned with what other people may think; they further attribute intentions to indifferent actions of others, more and more without judgment or attempt at verification of their suspicions.

d: The deterioration type. In cases of dementia præcox we find over and over an account of frequently perfectly exemplary childhood, but a gradual change in the period of emancipation. Close investigation shows, however, often that the exemplary child was exemplary under a rather inadequate ideal, an example of goodness and meekness rather than of strength and determination, with a tendency to keep to the good in order to avoid fights and struggles. Later religious interest may become very vivid, but also largely in form; a certain disconnection of thought, unaccountable whims make their appearance, and deficient control in matters of ethics and judgment; at home irritability shows itself, often wrapped up in moralizing about the easy-going life of brothers and sisters; sensitiveness to allusions to pleasures, health, etc., drive the patient into seclusion. Headaches, freaky appetite, general malaise, hypochondriacal complaints about the heart, etc., unsteadiness of occupation and inefficiency, day dreaming, and utterly immature philosophizing, and above all, loss of directive energy and initiative without obvious cause, such as well-founded preoccupations, except the inefficient application to actuality. All these traits may be transient, but are usually not mere "neurasthenia," but the beginning of a deterioration, more and more marked by indifference in the emotional life and ambitions, and a peculiar fragmentary type of attention, with all the transitions to the apathetic state of terminal dementia.

Just as the traditional theories of temperaments or constitu-

tions have served in a system of pathology of the past, such an attempt as the one offered here must rise and fall with its empirical usefulness. It seems to me to have several points in its favor. It aims at definitions of a nosological character, etiological as far as is warranted by the facts and stimulating in the direction of more precision in etiological investigation; yet at the same time careful to remain on the safe ground of clinical description. It is open to many supplements, and it will be an especially grateful task to push the inquiry of individual make-up along the lines of changes of constitutional make-up due to traumatism, to toxic influences, to sexual insufficiency, to the prevalence of certain thought-habits (especially the estrangement with actuality in the form of occultism), and under the influence of the period of involution and senescence.

It must, of course, be our ambition gradually to reduce the types of constitution to entities produced by definite conditions instead of simply classing them in a descriptive way. There will, however, always remain a residuum which resists etiological classification. Yet even there we must not be too easily tempted to turn to the problems of heredity and generation before we have made a thorough search of the patient's own life.

THE PSYCHOLOGY OF FOOTBALL.

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The ethics of football is a well worn theme; not so its psychology. It may be hoped that there will not be so much disagreement about the latter as there is about the former. A pessimistic writer in the *Contemporary Review* finds the English people on the verge of ethical pandemonium owing to the debasing influences of football. Teams advertise for players and buy them like chattels. Players sustain permanent physical and moral injuries. The spectators, under the excitement of a great game, become hoodlums, exhibiting violent partisanship and gross profanity, bestowing idiotic adulation upon the victors and heaping abuse upon the referee, restrained oftentimes only by the players themselves from inflicting upon him actual bodily injury. But a writer in the *Forum* finds in football a humanizing and elevating agency, "a school of morals and manners." It cultivates temperance and self-control, vigor and agility of body, quickness of perception, readiness of resource, manly courage, skill in planning, obedience, co-operation, *esprit de corps*. In the crowd of forty thousand spectators, we see an orderly, well-dressed, cultivated mass of humanity, composed of brave young men and beautiful young girls, innocently witnessing a contest whose issue is known to depend upon the temperance, bravery and self-control of the player.

There is apparently a slight difference of opinion here. The present writer, however, is for the moment as indifferent to the good or evil of football as the seismologist is to the ethics of the latest earthquake; but he finds in the phenomenon of football itself the opportunity of the psychologist and the sociologist. It would, indeed, be a valuable contribution to these sciences if we could discover the motives which draw such throngs of people to witness our football games.

We understand fairly well the impulses which determine a

man to work for bread or steal it, to scramble for money, fortune, social position, the favor of woman; but what is the motive which prompts the English workman to spend fifty-five minutes of his precious noon recess in watching a local football game, devoting five minutes to his dinner? Or why does the busy professional man, leaving his office, journey a hundred miles to see an intercollegiate match lasting an hour and a half? At a recent Minnesota-Wisconsin game, fourteen thousand spectators from all parts of the Northwest watched the game. At the Yale-Princeton and Yale-Harvard games in late years, there have been sixty or seventy-five thousand spectators present. At the international football match at Glasgow, in 1902, there were, according to the press reports, seventy thousand people on the grounds. Moreover, a great crowd gathered without the gates and, unable to gain admittance, broke down the barriers, leading to a panic in which twenty-one persons were killed, and two hundred and fifty seriously injured. The game proceeded, however. A writer in the *Nineteenth Century* for October, 1892, says, "thrice during the last season, the writer witnessed matches in violent snow storms; and in one of these, with snow and slush ankle deep on the ground, the downfall was so severe that a layer of more than an inch of snow accumulated on the shoulders and hats of the enthusiasts, who were packed so closely together that they could not move to disencumber themselves."

How shall we explain the peculiar fascination of this game? It is not due to intercollegiate or international rivalry, for other intercollegiate contests such as debates or contests in oratory, which are more in accord with the purpose of the institutions they represent, may perhaps after persistent advertising gather five hundred people at twenty-five cents admission; nor may it be explained as a fad which for the moment engrosses the attention of the crowd, for football has been played in England since the thirteenth century. From the fourteenth to the seventeenth centuries, it flourished under the most bitter opposition, repressed neither by the proclamations of the kings nor by the frowns of the nobility. In those days it was a game of the people, played through the streets, in a very rough-and-tumble fashion, but there was some strange

fascination about it overcoming all opposition. The Puritans, indeed, succeeded in stifling the game; but its history in England in the century just closed shows how it flourishes when freed from all opposition. Despite the Englishman's loyalty to cricket, his national game, football has swept the country with a virulence unknown even in America.

Evidently there is some great force, psychological or sociological, at work here which science has not yet investigated. To be sure we have had lately a psychology of play. Herbert Spencer considered it not without importance to ask why children play, and more recently Dr. Groos has given us his two suggestive books on the play of animals and man.

Why, then, do children play, and why do their plays take the forms of tag, hide-and-seek, ball, marbles, tops and kites? Why do grown-ups play and why do their plays, or sports as we call them, take the forms of hunting, fishing, yachting, horse-racing, baseball, football, cricket, tennis, golf, billiards, dancing, fencing and prize-fighting? In particular, why do some of these sports, such as football, baseball or horse-racing, appeal so much more powerfully to the people than others, such as hockey, croquet or checkers? To many it may appear that these questions are unanswerable, or that they are matters of course, just as once was said about eclipses or gravitation. The psychologist, however, must assume that they are answerable, at least theoretically. The play of children is no longer regarded as a meaningless way of passing time. Not only play itself, but every special form of it, has just as much meaning in relation to the life history of the race as has any bone or muscle of the body.

The familiar Schiller-Spencer theory of play, explains it as due to the expenditure of surplus energy. The frolicking colt, frolicking kitten, or romping girl is working off surplus nervous energy in activities not directed to any serious end, but serving nevertheless to give needed exercise to growing muscles. Dr. Groos has subjected this theory to a rigorous criticism and found it wanting. His own theory, now well known, is the "practice and preparation theory." He believes that play is an instinct, having for the child no conscious end beyond the pleasure of it, but being in reality a discipline exer-

cising every faculty for its future serious use. Childhood, in fact, exists in order to prepare the young through play for the duties of earnest life. For instance, the Indian boy plays with the bow and arrow, so far as he is concerned merely for the pleasure of it, but in reality it is an indispensable instinct without which he could not gain the necessary practice for his later serious duties.¹

It is not the place here to enter upon a criticism of these theories. They embody a certain amount of truth, but they fail to take into account the rich anthropological meaning of play. It is only from the standpoint of anthropology that the plays of children or the sports of men can be understood. The comparison of children's and adults' plays with the activities of primitive man throws a flood of light at once on the whole subject. Haddon, for instance, has made a painstaking study of the history of the kite and the top. Kite-flying is found to be an almost universal and very ancient custom, being traced back to the primitive Indonesian people among whom it was probably a religious ceremony, the kite being a symbol of the soul. Tylor gives a long list of children's games which he shows to be merely survivals of divinatory and other practices of early savage man, such for instance as casting lots, throwing dice, games of forfeits and games with common playing cards. The mental habits of our ancestors survive also in the charms and talismans and familiar superstitions of children. One recalls the magic formula used by Tom Sawyer for driving away warts: "You got to go by yourself to the middle of the woods, where you know there's a spunk-water stump, and just as it's midnight you back up against the stump and jam your hand in and say:

Barley-corn, barley-corn, injun-meal shorts,
Spunk-water, spunk-water, swaller these warts,

and then walk away quick eleven steps, with your eyes shut, and then turn around three times and walk home without

¹ Consult also Wundt's theory that human play "at least in its simpler forms,—*e. g.*, in the play of children,—is merely an imitation of the actions of every-day life stripped of its original purpose, and resulting in pleasurable emotion." *Lectures on Human and Animal Psychology*. London, 1894. Lecture XXIV, Section II, p. 357.

speaking to anybody. Because if you speak the charm's busted." According to Bolton, the counting-out rhymes of children are survivals of the practice of sorcery. It is possible that marbles, jackstones and ball all have some connection with early religious rites. The punctual seasonal return of these games adds force to this suggestion. The peculiar fascination of the ball, almost the first plaything of the child, and persisting in one-old cat, baseball, football, roley poley, cricket, croquet, tennis, hockey, lacrosse, polo, basketball, billiards, bowling, golf, pingpong, and many other games, can hardly be explained except on anthropological grounds.

The younger the child, the older the racial epoch represented by his mental habits. These are often echoes from the remote past recalling the life of the cave, the forest and the stream. The instinct exhibited in infancy, as well as in boyhood, to climb stairs, ladders, trees, lamp-posts, anything, is an echo of forest life; the hide-and-seek games which appeal so powerfully even to the youngest children are unconscious reminiscences of the cave life of our ancestors, or at least of some mode of existence in which concealment from enemies, whether human or animal, was the condition of survival; while the instinct of infants to gravitate towards the nearest pond or puddle, the wading, swimming, fishing, boating proclivities of every youngster, point back unmistakably to a time when our fathers lived near and by means of the water.

Again, the ancient life of pursuit and capture persists upon every playground in the familiar games of tag, blackman, pull-away, and a hundred others. Indeed, for the exhibition of this instinct, no organized game is necessary. Sudden playful pursuit and flight are seen wherever children are assembled. The ancient life of personal combat is mirrored in the plays of children in mimic fighting and wrestling. The passion of every boy for the bow and arrow, sling, sling-shot, gun, or anything that will shoot, is merely the persistence of deep-rooted race habits, formed during ages of subsistence by these means.

There was a period in the history of man when he lived in close relation with and dependence upon wild and domestic animals. The horse and the dog have even until recent times held a particularly prominent place in the development of

human culture. This period is reflected in many forms in the child's life: his nursery tales are largely animal tales; his first picture book is an animal book; his first words are often names of animals; his toys are mimic animals, and many of his plays are animal plays. The former dependence of man upon the horse is seen in the instinct of the child of to-day to play horse and to ride a rocking horse, or in default of this a stick or stair rail. The musical instruments of the child are not those of to-day but of former ages. Anthropologists tell us that the first musical instrument was the rattle, formed by enclosing pebbles or small stones in a sack made of skins, and that after this followed the drum and the horn. These are the first instruments of children.

These illustrations could be multiplied indefinitely. They show the inadequacy of the Groos or Spencer theories of play, for none of the plays of this class have much to do in preparing the child for the life of to-day, or in giving him special practice for his future work. We ourselves are so much slaves of the past in our habits of thought that we do not easily realize how far from the actual life of the present is this play-life of the child. The real world of to-day is that of the laboratory, the school, the library, the bank, the office, the shop, the street, the factory, the farm and the railroad. Notwithstanding the child's strong imitative bent, his world, as shown in his tales, his dreams and the plays he loves best, is that of the forest, the stream, the camp, the cave, the hunting ground and the battlefield.

But what is the explanation of this evident and striking parallelism between the plays of children and the serious life of primitive man? To use a biological term, it is known that the child 'recapitulates' the life history of the race. Just why he does so, biologists are not able to say; but the evidences, particularly in embryology, are striking enough. So far as concerns the plays of children, the explanation may not be far to seek. If we look upon the history of man as a development of the will, as an advance by means of effort, attention and concentration, it is easy to see that these later and more difficult achievements are ill-fitted to the immature child. He must, to be sure, be physically and mentally active, but his activity

will be along the lines of least effort, that is, of old race habits. The child is "the heir of all the ages" and inherits at birth the old time-worn brain-paths whose use makes little draft upon his easily fatigued nerve centres. By and by he will have to check these primitive tendencies, and by education and effort to bring the newer and higher centres into use. So without will, effort or fatigue, he follows the manner of life of his savage or half-savage ancestors.

From the vantage ground thus gained in the study of children's play, is it possible to explain the psychology of adult sport in general and of football in particular?

Obviously none of the theories of play hitherto proposed will apply to the sports of grown up people. The intense interest in a football contest which so fascinates a crowd of men that they will sit for two hours in falling snow, certainly is not due to Spencer's "superabounding energy," nor could Groos explain it as a "practice and preparation" for life's duties. It is not, indeed, to be hoped that any one principle will be discovered explaining all forms of sport. A part of the charm of tennis and golf is no doubt due to the recreative power of exercise and fresh air after tedious confinement in an office or schoolroom; but it is obvious that exercise and fresh air will not explain the fascination of football, baseball and horse-racing to the spectators, nor the attractiveness of the circus and amphitheatre to the ancient Romans. Other special sources of pleasure might again be found in other pastimes, such as dancing, skating, riding, driving, bicycling, or in the diversions of the modern theatre and opera, but none of these sources will explain why a football game will bring together fifty thousand people, while a baseball game attracts but ten or twenty thousand, and the most world-renowned singer, actor or musician scarcely as many hundred. The sports which are really most attractive to the people, as measured for instance by the space given to them in the daily press, can no more than children's plays be explained apart from anthropological grounds. Let us then make the hypothesis, for the moment certainly unverified, that adult play like that of children is reversionary, resembling the serious activities of earlier times. First, what reason is there in such an hypothesis, and secondly, how far do the facts support it?

The progress of civilization has been a slow, painful, upward striving in which the motive force has been the human will, the specialized form, it may be, of the greater cosmic will. The actual exhibition of this upward striving is what we call work. Effort and tension are its conditions. Its mental accompaniments are will and concentration. But this upward movement is not continuous or uniform. The curve of development is always broken, the steep ascents being followed by plateaus or depressions. This law no doubt holds true of cosmic, social and individual progress, in all of which lapses and relapses interrupt the forward movement. In the alternation of work and play, we have an illustration of the law as seen in miniature in the daily life of the individual. In play the mental activity must be of such a kind as to give the greatest possible relief to the higher brain centres involved in work. It is a well known law in psychology that the last mental powers developed are the first to suffer from fatigue. Play, therefore, if it is to serve the purpose of rest and recreation, will naturally involve the old time-worn brain paths and appear as the exhibition of half latent instincts. At least, this will be so far true as that those sports which involve these latent instincts will give the greatest relief from fatigue. We may expect to find, therefore, that the play of adults is in a way reversionary, recalling the serious pursuits of former days. It uses the older brain paths, allowing the newer and higher centres to rest. We may expect to find, furthermore, that in proportion as the sport is primitive, so much greater is the rest and recreative power and consequently the pleasure found in it. Genuine adult play is therefore a kind of "relapse," affording the sweet rest and abandonment peculiar to the relapse. It is a sort of unconscious reminiscence, with its own peculiar joy and delight.

It will be seen at once that the facts relating to adult play lend striking support to this theory. We still practise the same serious labors of our primitive ancestors, but we call them sports. We recall first that some form of *outing*, be it hunting, fishing, camping, or boating, is the most common kind of sport and affords the most satisfying recreation. The tired business man or college professor reverts in his vacation

to the fields, the forest, the stream, or the seaside. The tent, the gun, the rod and the canoe have not only a strange fascination for us, but a hitherto unexplained recreative power. Even when our respite is limited to a half day, we take our supper in a basket and go out to the lake or riverside and cook our eggs or boil our coffee over the campfire in quite the primitive manner, or possibly we revert for a few hours to the life of the canoe or sailboat.

The animal cult of our forefathers is seen in many forms in the sports of to-day, as in horse-racing, hunting with horses or dogs, devotion to luxurious stables, kennels, or lofts, in horse shows, dog shows, cat shows, pigeon shows, or in the mere keeping of domestic pets for pleasure. One of the most popular and exciting of all forms of sport is racing. In horse-racing and foot-racing we have the survival in the form of sport of what was once a condition of life. Mere speed of foot or horse was a quality of vital importance at one time in our history, but of little or no importance now when survival depends upon wholly different powers. So instinctively do we admire swiftness of foot, that we hardly realize how far apart it is from the actual competitive life of to-day, that of the bench and bar, the legislative hall, the office, shop, and railroad. No illustration of the persistence of ancient instincts could be better than that of the effect of a horse-race upon the spectators. The emotional disturbance is out of all proportion to the actual importance of the event before us, which is indeed almost wholly without importance in its relation to the world of to-day. There are many people who cannot even read a vivid description of a horse or chariot race, without curious chest disturbances, an index of excessive emotion.

The law that the serious activity of any social epoch is some ages in advance of the sports of that epoch is well illustrated in the circus and amphitheatre of the Romans, and in the bull fights, cock fights and prize fights of modern days. To speak only of the former, when we recall the sports of ancient Rome, we see at once that in sport we have a sociological factor of the greatest importance, and a profound psychological problem. Juvenal's phrase "bread and games" has become familiar. The popularity of any emperor was nearly proportional to his

liberality in the matter of games and spectacles. Emile Thomas says: "After the sack of Rome by Alaric, the miserable remnant of the original inhabitants and the peasants who flocked in from the environs to the number of ten thousand, loudly demanded games in the circus, which had to be celebrated among the smoking ruins." The Colosseum, whose magnificence receives a new meaning from our point of view, accommodated eighty-seven thousand people. The Circus Maximus was one of the most imposing of Roman structures. There is good authority for the statement that four hundred and eighty-five thousand spectators were in actual attendance at once upon its spectacles. The shouting could be heard in the suburbs of Rome. The upper wooden seats collapsed at one time, killing eleven hundred people. Rome had theatres, too, but the largest of these, that of Pompey, had seats for only forty thousand spectators, and despite the political interest which attached to many of the theatrical exhibitions, we must believe that the interest in the theatre was insignificant when compared with that of the amphitheatre and circus. Trajan gave a single entertainment lasting one hundred and twenty-three days.

Now what was the character of these amusements which so fascinated the Roman populace? They were horse-races, gladiatorial combats and the exhibitions and contests of wild animals. The anthropological meaning of the horse-race we have already considered. In the gladiatorial combats we see the hand-to-hand encounters of primitive man, almost as far removed from the actual work-a-day world of the Romans as it is from ours. In the display of wild animals and in their deadly combats with each other and with man, we see mirrored in Roman sports the old life of the forest and plain. The mood of the spectator at the Colosseum changes, too, to suit the character of the spectacle, and for the time he is no longer the civilized Roman of the second century but boisterous, cruel, intoxicated with the sight of blood. So strong was the craving for the old feral scenes that professional hunters were kept in the remotest parts of Asia and Africa to capture alive every species of beast to be exhibited and killed for the amusement of Rome. Eleven thousand animals were produced by

Trajan at a certain spectacle. Much has been said about the brutalizing effect of these games upon the Romans; but if we have correctly outlined the psychology of sport, we see in such games as these not a brutalizing agency, but an afterglow of brutality left behind. The modern circus, menagerie and zoological garden offer similar entertainment on a smaller scale, and appeal to the same instincts.

As a psychological problem, football would have to be considered both from the standpoint of the players and that of the spectators. The two problems are different, and it is the latter that now chiefly concerns us. If, however, we were to study football from the standpoint of the players, we should find that to some extent the Groos theory of play would apply to it. That is, we may believe that the peculiar fascination of the game for the players is due to the fact that it does indeed furnish a certain practice and preparation to the young for life's later duties. It gives training in endurance, courage, hardihood, co-operation, obedience, promptness and decision. It develops the physical powers and so indirectly lends support to the mental forces upon which the struggle for existence now turns. While these benefits are evident enough, it is doubtful whether they explain satisfactorily to any one the fascination of football for the players. To be sure, in football as it is now practised, especially in professional football, the dramatic element is predominant and the significance of the game is determined to some extent by its relation to the spectators. Thus far it ceases to be play, and becomes a form of work, the end being to win a certain number of games or to gain a certain amount of applause, fame or money. But probably no football player would be satisfied with either of these explanations. Football is in itself great sport, at any rate for young and non-professional players, and the fun is due neither to the benefits derived from the game nor to the presence of the spectators. The player himself would probably not be able to give a very satisfactory explanation of the pleasure of the game except to analyze it into the pleasures of exercise, competition, co-operation, victory and so forth. These and similar sources of pleasure are, however, present in many other games not so attractive as football; and it is evident that the peculiar attractive-

ness of football is due in some measure to the joy of rude personal encounter, face to face opposition of two hostile forces, swift flight and pursuit, kicking and catching the ball, and that the explanation of these unique pleasures must rest upon anthropological grounds. The game is more sport because the activities are more primitive. The anthropologist, moreover, discovers other primitive features in the game and will hardly admit that their presence is accidental: for instance, the bare heads and long hair; the dust and dirt and grimy faces; the Indian-like blankets worn by the players when at rest; the colored and decorated suits;¹ the primitive character of each part of the suit itself, such as the sleeveless canvas jacket, the loose moleskin or khaki trousers extending only to the knee, and the moccasin-like elk-skin shoes; the quick recovery from injuries; the possibly symbolic meaning of the ball; and finally the primitive character of the game itself, resembling as it does a scrimmage of savages.

But to the student of the psychology of sport, the peculiarly interesting problems of football relate to the game from the spectators' point of view. Whether it be work or play for the participants, it is sport pure and simple for the onlookers. The anthropological theory of play is brought into clear relief when we compare football with baseball. An analysis of the two games shows that they have many elements in common. The greater enthusiasm evoked by football must be due to qualities not present in the other game. During a recent baseball season, the writer attended a National League game at Boston. The attendance was small and the enthusiasm moderate. Sitting beside a man who looked like a veteran sport, the writer ventured to ask him why football drew larger crowds than baseball. He had evidently never thought of this question before but he said: "Well, it's only once a year. It's a college game, and [his eyes flashing and face working] well, it's for blood; it's more fun, by ———, than you can shake a stick at!" Further questioning brought out only the further

¹The development of dress has been steadily towards plain and sombre colors. In civilized countries, men now dress almost exclusively in grays, browns, dark blues and blacks. Women and children still wear the more gorgeous raiment of primitive man.

answer: "It's a sporty game,—very sporty. As the fellow said, 'Take a prize fight and multiply it by eleven!'" In reply to the question why football draws larger crowds than baseball, a college man said: "Well, football is more dramatic,—more like a fight."

These answers accord well with the anthropological theory. In this game more than in any other, except those of the Roman amphitheatre and their modern representatives, there is reversion to aboriginal manners, and hence a more complete relapse into latent habits, more perfect rest of the higher brain centres, more thorough-going rest and recreation. The game is more brutal, that is, more primitive than others. The scene before us is the old familiar scene of ages past. The lively chases for goal, as for cover, the rude physical shock of the heavy opposing teams, and the scrimmage-like, *mêlée* character of the collisions awaken our deep-seated slumbering instincts, permit us to revel for a time in these long restricted impulses, relieve completely the strain of the will, and so serve all the conditions of recreation. The game thus acts as a sort of Aristotelian catharsis, purging our pent-up feelings and enabling us to return more placidly to the slow upward toiling.

By inner imitation the spectators themselves participate in the game and at the same time give unrestrained expression to their emotions. If at a great football game any one will watch the spectators instead of the players, he will see at once that the people before him are not his associates of the school, the library, the office, the shop, the street or the factory. The inhibition of emotional expression is the characteristic of modern civilized man. The child and the savage give free expression in voice, face, arms and body to every feeling. The spectators at an exciting football game no longer attempt to restrain emotional expression. They shout and yell, blow horns and dance, swing their arms about and stamp, throw their hats in air and snatch off their neighbors' hats, howl and gesticulate, little realizing how foreign this is to their wonted behavior or how odd it would look at their places of work. The excitement of the spectators cannot be explained by the importance of the scene before them, for, as in the case of the horse-race, it has little or no relation to the serious life

of the present; but its scenes are those which were once matters of life and death. The prevalence of gambling in connection with football as well as in horse-racing, prize fighting and other popular sports illustrates the reversion to primitive morals, accompanying the return to primitive activities.

In conclusion, it should be observed that the psychology of football and similar sports does not teach that in these games there is a return to savagery. There is a momentary return in the form of sport to the serious manners of former days in order that in the serious affairs of to-day, these manners may be the more completely left behind. The intense passion for such games is in itself an indication that they answer to some present need. This need we have already indicated in the psychology of work and play. In those countries where serious life is taken most seriously, as among the Anglo-Saxon nations, there is exhibited greater occasional abandonment to those sports which afford the greatest relief from mental tension.

RETROACTIVE AMNESIA: ILLUSTRATIVE CASES AND A TENTATIVE EXPLANATION.

By Professor WM. H. BURNHAM, Clark University.

The German philosophers, Fries and H. Schmid, taught that not the persistence of ideas which have always been in consciousness but the forgetting of them requires special explanation. Sir Wm. Hamilton, who had no patience with physiological explanations of memory, maintained the same. Not memory but forgetting is the mystery. Modern science takes a different view, and yet in the phenomena of amnesia it finds the most promising opportunity for the study of the conditions of memory. Specially interesting are those cases resulting from shock or disease where the forgetfulness extends to events preceding the cause of the amnesia. Such cases may, for convenience, be divided into two classes, cases of retrograde amnesia and cases of retroactive amnesia. In the former class I shall include those cases where memory is obliterated for a relatively long period preceding the immediate cause of the amnesia, and in the latter those cases, usually the result of shock, where the amnesia extends to only a short period, a few minutes or a few hours, immediately preceding the accident.

The purpose of this paper is to present one or two cases of retroactive amnesia and suggest a tentative explanation; but first a brief discussion of retrograde amnesia and kindred phenomena may be helpful.

THE PHENOMENA OF DISSOCIATION.

Everybody, perhaps, has had the experience of trying to recall a forgotten name; the vague glimmer of it haunts us; we know it is there, but we cannot get it; for the time being it is dissociated from our dominant train of thought; but the proof of our possessing it is furnished later on when it comes, perhaps spontaneously, into consciousness. Equally common, perhaps, is the experience of planning to do something,—to attend to

some errand, or perform a minor duty, or the like—and then in the multitude of cares forgetting what was to be done. The tantalizing feeling of knowing that we ought to do something and of not knowing what it is persists. Here again the thing forgotten is merely dissociated from our present train of thought. A mere change of scene or diversion for a few minutes may be quite enough to give us the cue and bring it back again to memory. Still again upon going to a new place and entering upon new duties we frequently forget such a large part of the routine details of our former life that we seem for the time being almost different personalities.

All these familiar experiences are simple forms of dissociation. The following is more rare and on the border line probably of the pathological. The case is reported by a professional man who had been suffering for some months from overwork and probably nervous disease as well. I give it in his own words:

"You have often experienced the sensation of *oldness* instead of *newness* when in a strange place—that is, the feeling that you had seen the same before. Did you ever have the converse of that feeling? It has happened to me several times of late. Objects the most familiar all at once seemed wholly strange and altogether unrecognizable as never having been seen before. One morning while on my way to the city I left the house and walked toward the place where I was to take the street car, a distance of four short blocks, a route traversed by me almost daily since I returned from——, and one quite familiar to me before I went away. I walked along absorbed in thought when I suddenly found myself in a strange place. I looked ahead, to the right and to the left, and then turned and looked back, but in no direction could I see anything I had ever seen before. I walked back to the street I had last crossed and looked up and down it but could see no familiar object. I then retraced my steps to the place where I had first stopped and looked about as before with the same result. Still it did not seem possible that I could have gone astray, as I could not have found strange ground by following the street I had started on without going a considerable distance, and I did not think I had been walking more than two or three minutes,

and I did not think I had turned from that street. As there was nothing in sight which I could recognize I tried to recall the looks of the houses on my usual route and to compare them with those in sight, but I could not visualize them sufficiently to make a comparison. I, however, remembered a church which if I were on the right road should be about a block away on my left, with an open square between me and it, and I looked for the church. There appeared a church just where I should have located it but it was one I had never seen before. I stood and studied it critically and analyzed its appearance, its size, color, shape and proportions, and though I scrutinized each part I could have sworn that I had never seen it before. I concluded to go on, and did so, coming to a street with car-tracks within less than a block, but there was nothing familiar, in fact nothing I could recognize as having seen it before. I waited till a car came along and read on it the sign of the line of cars I was accustomed to take and so boarded the car and got safely to the city. I have since that had the same feeling several times in the city and once while on the car I was so certain that I had taken the wrong car, or gone too far, from the strangeness of everything I saw through the car windows, that I stopped the car and got off finding myself in an utterly strange place. I tried to find my location by reading the names of the streets on the corners but could not find the signs, so I inquired at a saloon at one corner the names of the intersecting streets and found it was a corner I had stopped at at least a hundred times. I was not in the least excited on either of these occasions but the first time I was very much surprised. May not such an experience, with concurrent loss of memory, account for some of the instances where people go away from home, disappearing suddenly, and finally turning up after extended travels with no recollection of going away?"

Such phenomena of dissociation vary from the simple and familiar experiences first mentioned through all degrees of elaboration and complexity up to the well-marked cases of double personality reported by Azam, Prof. James and others. Dr. White of the State Hospital at Binghamton, N. Y., has

recently reported a case¹ which illustrates very well the gradual development of such phenomena of dissociation from the simplest forms up to a clear case of secondary consciousness. The following is an abstract of his detailed report:

J., girl, aged 14. Lost both parents in infancy, mother died in a hospital for the insane, brought up with a brother and two sisters much older than herself. Apparently normal to age of ten. Puberty came then with radical change of character, became quiet and sedate, but continued successful at school. When 11½ she heard from the lips of a neighbor the gruesome details of a suicide. That night the thought came to her,— what would her folks think if she killed herself. Her brother spoke crossly to her that evening, and she went to bed to cry and dream of the suicide; but she was soon awakened by the slamming of the door and her brother's footsteps and heard him ask: "Is she dead?" Then her brother locked the door and went away leaving her in the house alone. Frightened and crying, she walked the floor and feared that her sister was dead, and the desire to kill herself first came into her mind. Later she learned that her cousin had been run over by a train, but she continued to worry, however, until her sister returned at 2:30 A. M.

During the next year or more she forgot the old woman's story, but the idea of suicide remained, and she would frequently say, "Well, maybe you will be sorry, I shall kill myself." Then she had trouble with her brother and the idea of homicide was suggested. A year later she became attached to a young man and was bitterly opposed by her family friends. Trouble with her eyes and headaches ensued. Soon after she made several abortive attempts at suicide. While in this condition one evening she attended a party where a young man shot himself, and she heard the shot and saw the body. After one or two other unfortunate events and several unsuccessful attempts at suicide, she had la grippe, and on recovering, fell, striking her head severely and remaining unconscious most of the time for several days.

"During all this time," continues the report, "she acted

¹Sidis, Boris: Psychopathological Researches, New York, 1902, pp. 125-158.

strangely, often did not know where she was or recognize those about her. On the third day of the attack she was taken home, and on the afternoon of the fifth day suddenly came to herself, with absolutely no recollection of what had occurred. The last thing she remembered was lying on the sofa in her aunt's house at N. From then on everything was a blank, even her journey home having left no trace in her memory.

"This was her first attack of what we shall hereafter call her secondary state, a condition from which she rallies with absolutely no recollection of the events that occurred during its ascendancy."¹

Such was the patient's condition when admitted to the hospital. She suffered from well-developed suicidal obsession and erythrophobia and was easily hypnotizable. After treatment for six months she was permitted to leave the hospital on August 29, 1901, on thirty days' parole. On September 28, 1901, she was discharged. "Since that time" says Dr. White, "I have been in constant correspondence with her and have every reason for believing that she is perfectly well in every way."² The method of treatment I give as reported in Dr. White's own words:

"The principle followed in this case was that of bringing together,—reassociating—what had become dissociated: synthesis of the dissociated subconscious states. All the details of the events for which the patient was amnesic were thoroughly traced by use of hypnosis and hypnoidization, and were then united to her upper, personal consciousness, so that she is now in full possession of all the facts. These facts, obtained from her in this way, were subsequently verified by numerous conversations with different members of her family.

"It is noteworthy in this connection, that all of her acts and sayings which had previously seemed to have no foundation in reason, but, on the contrary, had every appearance of being quite incoherent, could be traced in each instance to an adequate cause, and thus what appeared as chaos on the surface was reduced to order."³

¹ *Loc. cit.*, p. 146.

² *Loc. cit.*, p. 156.

³ *Loc. cit.*, p. 153.

The one distinguishing characteristic of cases of dissociation is that ideas forgotten are potentially related to consciousness, directly, perhaps, to a secondary or subliminal consciousness, indirectly to the dominant consciousness. By means of hypnosis, of change of environment, or some other means, they may be revived, and usually may be brought into association with the dominant consciousness. Many cases of so-called retrograde amnesia are of this class or closely related to it. Not all, however, are to be classified in this way. In many cases the memory is hopelessly obliterated.

RETROGRADE AMNESIA.

Although the usual method of classification of amnesias is unsatisfactory, I have no intention of attempting here any rigid classification, and would only say that the essential characteristic of cases of retrograde amnesia as distinct from phenomena of dissociation is that in the former case ideas of a certain modality (as in aphasia) or all those relating to a considerable period of the patient's life are obliterated, and for a relatively long period at least, cannot be revived by psychological methods.

It is often very difficult to determine whether the amnesia is a case of dissociation or not. Many of the cases of retrograde amnesia usually cited and those that have been reported to me seem to be at least closely related to the phenomena just described. The following case, the account of which I owe to Dr. Everett Flood of the Massachusetts Hospital for Epileptics, Palmer, Mass., shows this close relation of these two forms of amnesia.

"N. B. F., male, age 60. Patient has had the ordinary diseases of childhood. He has had epileptic convulsions since he was seven years old. He had been confined in Northampton Insane Hospital for about twenty years preceding Feb. 25th, 1902, when he was transferred to the Massachusetts Hospital for Epileptics.

"His father and mother both died of pneumonia, had one sister who was insane. Patient's present condition seems somewhat demented. He complains of having a poor memory, and is just recovering from an attack of muscular rheumatism,

and is somewhat emaciated, due to recent exposure and improper care.

"Nov. 11, 1902, patient escaped from this hospital and no trace of him could be found until Jan. 10, 1903, when a letter was received stating that he was at the Poor Farm in the town of Sturbridge, Mass. A physician and nurse from the hospital went to Sturbridge to bring patient back. Patient failed to recognize either the physician or the nurse and stated that he had never been in the Hospital for Epileptics. He did not remember having his eyes treated there or assisting in breaking the steers (an occupation in which he was quite expert). He did remember, however, having been in the Northampton Insane Hospital and of working on the farm there. His memory seemed to be entirely obliterated for a period of two hundred and sixty days, between Feb. 25th and Nov. 11, 1902.

"He did not know that his name was Fisk; but said it was Stebbins. He could talk quite rationally about what had occurred since he left the hospital. He said that for a time he was peddling books; although he could not remember the name of the man by whom he was employed. After that he worked on a farm until within three days before he was taken to the Poor Farm. The master of the Poor Farm stated that the patient told him that he was walking along the road, when a man whose name was Kelly came along with a team, and patient applied to him for work. Mr. Kelly took him into his team and carried him to Sturbridge, where he employed him selling books for a time.

"After patient was brought back to the hospital, he was quite weak for several days; but the loss of memory, which remained for a few days, gradually began to clear up, and at the present time he knows the names of the nurses and of each of the buildings.

"When asked to try to remember how he left the hospital, he stated that he found himself on a rock out in a field and that he got up and walked along in the road quite a distance, when a man with a team came along with whom he got a ride.

"The cook in the kitchen where he was employed previous to his escape, stated that he had acted strangely for a day or two before he ran away. It is very probable that this was a

pre-epileptic confusion, and that the morning of his escape he had a convulsion, and when consciousness returned he was out in the field on the rock.

"The chief points of interest are the confusion, which probably preceded a convulsion, and the total amnesia, covering a period of 260 days, which gradually disappeared when patient was returned to his familiar surroundings."

The following case of a very different character has been reported to me by a student:

The case is that of a young lady, age 17, rather mature for her age. She reports that she had fever (perhaps typhoid) at 3 years of age, spinal meningitis at 7, and diphtheria at 9. "In the spring of last year" (1902), she writes, "while attending the University I became exhausted through over-work. One afternoon when returning home something seemed to snap in my head and it went whirling. This itself is clear in memory, but how I got home and what happened in the next three days or in the whole preceding month are forgotten. Of course from what has been told me I know now about what did happen but it is still impersonal as a story. I have no memory of the lessons we studied, and though during the time I was sick and before it I wrote verses constantly, I do not know them now or recognize them as my own work. At the time age seventeen."

At present, she reports, that her health is good although not as good as before the experience of amnesia mentioned. In the spring her memory is rather poor, she can hardly remember things from one day to the other. This is due, she thinks, to the fatigue of the year's work; at other times her memory is unusually good.

Other interesting cases, notably that studied some ten years ago by Dr. Dana,¹ and the one recently reported by Dr. Cowles,² are accessible in current medical and psychological literature.³

¹ Dana, Charles L.: The Study of a Case of Amnesia or 'Double Consciousness.' *The Psychological Review*, Vol. I, 1894. New York, pp. 570-580.

² Cowles, Edward: Epilepsy with Retrograde Amnesia. A Medico-Legal Study of the Case of Amos D. Palmer. *American Journal of Insanity*, Jan., 1900. pp. 593-614.

³ See also Sidis, Boris: Psychopathological Researches. *Studies in*

RETROACTIVE AMNESIA.

The study of amnesia shows the great complexity of the problem of explanation, but it is helpful at the outset to have a simple preliminary hypothesis. This should be held tentatively, but the advantage of it is that it may save us from seeking unnecessarily for a complicated theory. Such a simple tentative hypothesis is here offered for those cases of backward-working amnesia which do not fall under the classes already mentioned, namely, for the cases where the amnesia extends to a few minutes, or at most a few hours preceding the cause of it, and to which I would limit the use of the term retroactive amnesia. That such cases stand in a class by themselves has been noted by Ribot.

"Temporary amnesia," he writes, "is also frequent in cases of cerebral excitement, and then represents a *retroactive* character, that is to say, the patient, when recovering from unconsciousness, has lost not only the recollection of the accident he met with (fall from a horse or a carriage, blow on the head, etc.,) but also the recollection of a more or less long period of his life *before* the accident. Dr. Frank Hamilton has reported twenty-six cases of this kind, which he communicated to the Medico-legal Society of New York (1875) and upon the forensic importance of which he lays stress. According to his opinion amnesia of events *before* the cerebral shock may extend over a period varying from five minutes or more to two or three seconds. It seems, therefore, that in order that a recollection may organize and fix itself, a certain time is necessary, which in consequence of the cerebral excitement does not suffice."¹

The following cases will serve as illustrations of this form of amnesia. The first is reported to me by Dr. Douglas Graham, of Boston, and I give it in his own words.

"One evening in Oct., 1900, at 9:15, I found myself in bed with my head sewed up. I asked my wife how I came there and what had happened. She said I was knocked down by a 'scorcher' and brought home in an ambulance by two police-

Mental Dissociation. New York, 1902, pp. 329. Also papers mentioned in the bibliography at the end of this article.

¹Ribot: Article on Memory, in Tuke's "Dictionary of Psychological Medicine." pp. 799.

men three hours before. A few minutes before that I had a confused recollection of people moving about my room and raising my arm,—the doctor took my temperature—and of trying to remember what had become of my accident policy.

"I have a dim recollection of getting off the car about ten minutes before I was knocked down by the bicycle. Then there was an absolute blank for about three hours.

"I asked my wife how I seemed when the policemen brought me into the house. She says I walked from the ambulance with a policeman on each side of me—steadyng me—and that I was very profuse in my thanks for their assistance, declaring all the time that there was nothing the matter with me.

"Three days after I called on the policemen and asked them how I appeared when they first found me. They said I was sitting on the sidewalk with blood running down my face, and they asked me if I could walk home— $\frac{3}{8}$ mile—I replied *no*, and I then asked them, who are you? who are you? like a drunken man. (I never was drunk in my life.)

"The bicycle struck me on the right hip and did no harm there; but there was a scalp wound 3 or 4 inches long over the left parietal bone. So I must have been knocked down like a ten pin, and turned a sort of somersault. When the doctors were sewing my scalp I asked them, Why don't you use cocaine? Why don't you use cocaine?

"Next day I was out with my head sewed up and felt unusually quiet, cool and collected in my upper story,—whether as a result of the bromide or the blow I cannot tell. I was disposed to think that the vigorous percussion or concussion acted as a powerful sedative—both at the time and after, not altogether in a harmful way.

"The following day—36 hours after the accident—I went to my office in Boston and attended to my duties as usual. I live in Brookline.

"It was almost worth the experiment to learn that I am such a hard-headed Scotchman, though I do not care to repeat it for that purpose.

"I am now 55 years old—weight about 150 pounds, of a rather nervous temperament, and enjoy pretty good health."

I add one more case reported to me a few years ago by a physician; but I cannot obtain the details nor vouch for it, as

the original report was mislaid; but even if imaginary, I think the account is typical and so worth giving.

A farmer spent his morning in his usual vocations. Then he shelled some corn, afterwards worked in his garden, then harnessed his horse and took a ride for a mile. At the end of this ride, he was thrown from his wagon and seriously injured his head. He remained unconscious for some time, and then when restored remembered the work of the early morning, dimly recalled the shelling of the corn, but remembered nothing of what occurred afterward.

How shall we explain such cases?

The theory I have to present is a very simple one. It has not been elaborated by any one so far as I know; but Ribot in the passage cited above suggests a similar view, and it is in harmony with our general knowledge of the physiological conditions of mental activity. It seems well at least to test a simple theory of this kind before resorting to anything more complex. It is briefly as follows: The fixing of an impression depends upon a physiological process. It takes time for an impression to become so fixed that it can be reproduced after a long interval; for it to become part of a permanent store of memory considerable time may be necessary. This we may suppose is not merely a process of making a permanent impression upon the nerve cells, but also a process of association, of organization of the new impressions with old ones.

During our ordinary life, as we may suppose, the physiological processes upon which the permanency of our impressions depends are continually going on. Hence, at any given moment some of our impressions received in the near past, say during the last twenty-four hours, are completely organized; others are nearly organized; others are partially organized; while still others have just been received. The time required for this process of organization may vary with different individuals and different conditions, but in all cases it seems to be necessary.

Now suppose a shock occurs which arrests these physiological processes in the nervous tissue. What will be the result? Not only will the mind be a blank for the period of insensibility following the shock, but no impressions will be remembered which were not already at the time of the accident suffi-

ciently well organized to make their persistence for a considerable interval possible. Hence, the amnesia will be "retroactive." Events immediately preceding the accident are entirely forgotten. This is illustrated in the case of the farmer cited. The mile ride, harnessing the horse, working in the garden, the events immediately preceding the accident are entirely forgotten. Others, that occurred earlier, such as shelling the corn, are dimly remembered; and only events preceding this are clearly recalled.

On the mental side an important factor in fixing an impression is probably the automatic repetition of it. This is seen in the case of people who think audibly, repeating words that they have heard; perhaps especially in the case of children; but where there is no such motor expression, nevertheless an automatic repetition of the idea very likely occurs. Now this mental process, as well as the correlative physiological processes involved in the fixing of impressions, are, we may suppose, arrested by the shock which destroys consciousness.

The second important factor on the mental side is the process of association, of linking the new with the old. This process of associative memory is perhaps the most fundamental fact in our whole psychic life. It has aptly been compared by Zanotti and Hume to the law of gravitation in the physical world. It is this that makes it possible to profit by experience. The possibility of this marks the first grand division in psychic evolution. Now great fatigue, excitement, unconsciousness, and narcosis arrest in varying degrees this process of association.

We do not know the nature of the physical processes correlated with these psychic acts of automatic repetition and association, but evidently time is required for them; and if the ideas in question are to become a part of the permanent store of memory, considerable time is needed. If these psychological processes of repetition and association and the corresponding physical processes are arrested by excitement or the like, then, as has been shown, we should expect to find the amnesia retroactive.

The essential characteristic of these cases of retroactive amnesia is that the memory is lost because it was never fully organized.

The amnesias of epilepsy are among the most common cases

of retrograde amnesia. They appear to vary greatly in character, all of the classes we have distinguished occurring. The study of them seems to corroborate the hypothesis here presented.

"Alzheimer, in an article on retrograde amnesia in epilepsy, refers to the numerous reports in the literature showing that after traumatism and hysterical attacks, a lapse of memory for a more or less protracted period preceding the traumatism or the attack may occur; this retrograde amnesia may be confined to individual acts, or a few hours, or it may comprise days and months; and, as it seems, it may sometimes take in the whole preceding life." He discusses also the retrograde amnesia occurring in conjunction with epileptic paroxysms.

"Alzheimer's cases were (1) a patient who had after attacks two retrograde lapses of memory; one that extended over four weeks and cleared up after one week's duration; the other covered a year and a half before the attack, and memory began to return about three weeks after it; (2) a patient whose amnesia extended over a week previous to a series of attacks occurring in three days, and cleared up twenty-one days later; (3) a patient who had a series of attacks within six days followed by an amnesia that extended back two weeks before they began, and which cleared up three weeks after the attacks."¹

Some of these cases referred to by Alzheimer are apparently cases of dissociation; others are cases of retrograde amnesia proper; others are perhaps of the kind we are now considering. He notes that the memories of the time of the paroxysm are "dream-like, indefinite, and incomplete," while those lost by retrograde amnesia are, when revived, "sharp, clear, and definite." This is precisely what we might expect, the former having never become really permanent memories because the processes or organization were interfered with by the paroxysm. And Féré has noted that different forms of amnesia may occur in the same patient and that the retroactive amnesia in several cases has seemed to vary with the severity of the attack.

Again "It is admitted," writes Dr. Cowles, "that uncon-

¹Allg. Zeitsch. f. Psychiatrie, LIII, pp. 483. Cited from résumé by Cowles, Edward: Epilepsy with Retrograde Amnesia. American Journal of Insanity, Jan., 1900, pp. 606, 607.

sciousness is a fundamental characteristic of epilepsy, and, as a rule, that the epileptic does not retain a single recollection of the acts performed during the impulsive crisis; but it appears sometimes that the patient is fully conscious of the acts he commits and remembers them. . . . The memory may also be patchy, with obscure and dream-like recollections, more or less clear and prolonged, of single acts or occurrences while in the epileptic state."¹

These apparent exceptions do not necessarily impair the truth of our hypothesis. Whether a certain degree of shock shall be sufficient to destroy certain memories or not, will depend upon the condition of the patient and the strength with which the given impressions were primarily fixed. Some impressions may have been attended to so intensely that they persist in spite of the shock.

Probably many cases of amnesia in epilepsy are of a mixed type. The interesting case (referred to above) studied by Dr. Cowles, for example, seems to me of this kind. It is doubtful if it can be explained, except in part, by the hypothesis here presented.

If this theory be true, it has great pedagogical suggestiveness. In pathological cases we see in simpler form the same processes that occur in normal memory.

There must be time for the processes of organization and assimilation to take place. This is further emphasized by the results found by Ebbinghaus. In learning his nonsense syllables, a given number of repetitions at one sitting was not nearly as effective as the same number of repetitions divided into several sittings. There must be time for nature to do her part. Without appealing to any mystical form of mental or cerebral activity it is clear that a night's sleep may be more effective in fixing a lesson in the memory than continued repetition. Hurry defeats its own end.

CONCLUSIONS

The following conclusions are suggested by this preliminary study:

1. The cases of amnesia due to shock or the like, where the

¹ *Op. cit.*, p. 605.

loss of memory extends only to a relatively short period preceding the cause of it, stand in a class by themselves and are to be distinguished, on the one hand, from cases of dissociation where it is possible by psychological methods to revive the forgotten memories, and, on the other, from cases of retrograde amnesia proper where memory is obliterated. To this class the term retroactive may conveniently be limited.

2. In normal memory a process of organization is continually going on,—a physical process of organization and a psychological process of repetition and association. In order that ideas may become a part of permanent memory, time must elapse for these processes of organization to be completed.

3. In cases of retroactive amnesia, as we have defined the term, the amnesia results from arrest of these processes of organization by shock or other cause. The memory is lost because it was never completely organized.

4. In normal memory these processes of organization are essential in order to fix impressions, and anything that interferes with them,—fatigue, hurry, distraction or excitement, hinders acquisition.

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THE STATE OF DEATH: AN INSTANCE OF INTERNAL ADAPTATION.

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The ordinary man is a drifting being; he swims complacently with the current. His natural business seems to be his adaptation to external circumstances, physical and moral. The reward of his supineness is usually what is accounted a fair share of success. He is a true representative of the world as seen through Darwinian spectacles.

Here and there, however, one meets a man, or a group of men, who claim to be a law unto themselves, who stand up against society as creative centers of energy, hardly condescending to accommodate themselves to physical necessities. Impelled by inner needs they strive for the realization of a type for which there is no external demand. Men such as these deserve the attention of the philosopher, particularly if he would know the purport of the forces at play in humanity.

In this paper I propose to describe, analyze, and point out the social significance of one of the most remarkable instances of the realization in an individual of an ideal in opposition both to external influences and to that, in himself, which may properly be called the *primitive man*.

There is a condition of relative simplicity of psychic life in which desires or impulses usually appear singly in consciousness and do not, therefore, enter into conflict with each other but pass on, unresisted, into action. Or, if several happen to meet, the conflict which arises is purely a matter of intensity, and not of quality. Certain things are liked more but not better. In a higher type of organization the conative movements are generally pitted against each other in a struggle for mastery on the ground of qualitative differences. In some men the impulses, inclinations and desires array themselves in two opposed groups and inner life becomes essentially the history of the defeats and of the victories of the group claiming

an absolute value. The strife, often greatly protracted and tremendously intense, ends in some of these men—Christian Mystics—in the production of a transformed personality as curious to the psychologist as practically important to society. They call it the *State of Death*. It is, in their mind, the successful result of their heroic efforts to obey inner promptings. On its practical side, it is nothing less than a serious solution given, experimentally, to the problem with which the salvation-philosophers, Schopenhauer and Hartmann, have entertained the world. It is defined thus by Jacques Olier, the founder of Saint Sulpice.¹ 'What is the State of Death? It is a state in which the heart cannot be moved in its core, and although the world offers honor, wealth and things of beauty, it is quite as if they were presented to a corpse without movement and without desire. The soul inwardly led may well be moved by external objects, but only superficially; it does not proceed from the interior which remains passive.'

Is this sheer verbiage, or is it a tolerably accurate description of a real condition not without deep significance? Let us turn to some other mystics for ampler information.

Mme. Guyon² describes minutely in her *Life* and in the *Torrents* the steps through which the soul comes to its mystical death. The 'natural' desires and impulses, together with the thoughts and images connected with them, must disappear. The 'Natural Man' must 'rot and be buried' and in the place of the passionate, assertive, eager, self-seeking person is to arise an empty, indifferent, passive creature prompted by harmonious impulses coming from beyond the self. When once the 'Natural Man' is dead, God is said to rule supreme. 'The three powers of the soul, understanding, memory and will have died.' The soul has found that 'repose in God' which Eckhart tells

¹From the *Souvenirs de jeunesse* of Renan, abbreviated.

²Mme La Mothe de Guyon (1648-1717), the propagator of Quietism in France, one of the ablest of the Christian Mystics, sorely persecuted by a party in the Gallican clergy headed by Bossuet. She differs in no essentials from the other Christian Mystics, but only in the more thorough manner with which she carries to their logical conclusions the tendencies and beliefs common to the group. She may therefore be used as its representative. A fuller study of her case will be found in my paper on Mysticism, *Revue Philosophique*, 1902, pp. 1-36.

us is the highest goal of man. 'The Self no more moves, it *is* moved.'

It ought not to be supposed that the bad desires only are to be suppressed. This self-surrender (*abandon*, of the French; *Gelassenheit*, of the German) involves the good as well as the bad. The Soul is to surrender entirely to its Saviour. The words of St. Paul must become literally true, "I live; yet not I, but Christ liveth in me." Thus Suzo in that amazing work, *The Book of the Eternal Wisdom*, calls the state of Salvation a *Nicht*, reached when every faculty, even reason, and not alone the lust of forbidden things, has been renounced. He warns the reader that the blessed state "is not for the many good people who wish from morning till night 'if God would only grant me this or that,' or, 'Would to God I were otherwise.' Resign yourself absolutely to the Will of God. In everything and in every circumstance say with Christ 'Father, not my will, but thine be done.' " And he adds, "he who desires what is outside of himself, or who sorrows because of what he finds in himself, has not yet reached the *Grund*, has not yet fully surrendered." The absence of effort, whether as striving or resisting, may be so complete as to throw the person for a certain period into a state of general automatism, and then we have the curious condition realized and described by Suzo, Ruysbroeck, St. Theresa, Mme. Guyon and many others, a peculiar trance-like state, a partial somnambulism, in which the disciple, altogether ruled by God's Will—to use his own terms—attends to his daily task more or less unconsciously.

It is evident that the significance of the transformation before us depends upon the meaning attached to that much used expression 'the Will of God.' Let us try and define it with some precision before considering further the state of Mystical Death itself. The orthodox Christian classifies the conative functions as the manifestation, either of the Will of the Natural Man or of the Will of God. The distinction exists under some name or other in all men who have reached the ethical plane. Duty, the Right, the Highest Good of the Greatest Number, etc., are respectively in different persons synonymous with the Will of God. But the meaning attached to these expressions changes somewhat with each person. Any one of the

great Christian Mystics, or, for that matter, any earnest Christian of the past or of the present, would give us accurately enough the meaning ascribed to the Will of God by the group of men under study.¹ The diagnosis made by Nicholas of Basle of the spiritual condition of Tauler is as good a concrete illustration as may be asked for.² He was 50 years old, an influential man, deservedly honored for his talents, his piety and his virtues, when Nicholas bluntly put before him his imperfections. 'Your trust is not exclusively in God and you do not seek Him alone, but you seek your own self,' said Nicholas. 'You trust in your own knowledge and in your own talents. There is vanity in you and love of ease. You are too much drawn to the creatures and, in particular, to a certain person whom you love with all your heart, immoderately. You have squandered your time in living for yourself.' That is the impeachment. Tauler must have been already aware of his deficiencies, for he pleads guilty and enters, of his own accord, upon a course of purification which none but a soul of the noblest temper could have endured. Let the outcome be what it may in this particular case, it is with the ideal that we are concerned. The goal set before Eckhart's disciple is the eradication of natural pride, of the misplaced pleasure felt, not in the achievement, but in oneself as its instrument. There is the source of evil. Therefore no more preaching to find delight in the sense of one's power and in the incense of compliments; no more discoursing to display knowledge or wisdom; no more inexact speech to call forth admiration or spurious interest; no more silence in order to keep people's good opinion when justice would require utterance; no more ambiguous smiles; no more false dignity—nothing but the unvarnished expression of the truth under the guidance of Christian charity.

After all, this ideal, the goal of our Mystics, is nothing else

¹In the first twenty-two chapters of *The Spiritual Nuptial* Ruysbroeck makes a typical picture of the virtues demanded of the Bride by the Bridegroom.

²The following is taken from an anonymous biographical account prefaced to twenty-five sermons of Tauler edited by Susanna Winkworth. It is asserted that the conversion there related is falsely ascribed to him. However that may be, its value for our purpose remains, whether it be the conversion of Tauler or of some one else.

than the Christian moral ideal taken without the usual compromises dear to the so-called practical man. And, in a wider view of the case, it is nothing more than the unflinching and consistent application of those ethical principles generally admitted, if not practiced, in civilized nations.

The significance of this ethical tendency is that it aims at replacing individual by universal motives, the private will by the larger will; it makes for action from *universalized motives*. Of all the spectacles offered by nature, I can think of none so majestic as this one: A little creature come to life with a pugnacious and even ferocious instinct of self-preservation and narrow self-increase at the expense of every one and everything, entering upon a battle to the death with his original self to transcend it, and ultimately, on the ruins of his initial nature, in an amazing act of self-renunciation, making the General Will his own and thus becoming the moral equal of God.

This lofty yearning may be as well and as strenuously expressed in the common details of life as in its larger relations. Food, dress, customs may serve to pit the individual against the universal; or, in religious phraseology, man against God. The littlest incident may be an epitome of what the ethical spirit in and out of religion strives to achieve, instead of being but a quixotic wind mill.

And now we may return to that curious State of Death and inquire first how it is brought about and, later, consider it in itself. The theory to which the Christian Mystics conform is the following.¹ It is not best to fight directly the 'Natural Man,' for the lower desires seem to feed on the resistance offered them. The creature must be passive in the hands of the Creator. 'Be content with the present moment as it brings you the eternal order of God. Whatever happens to you is God's will. Our action must be to place ourselves in condition to receive God's action. Give up every specific inclination, however good it may seem, as soon as you detect it, and place yourself in indifference, desiring God's will only. Abandon

¹ I follow chiefly and almost verbatim Mme. Guyon in *Le Moyen Court et facile de faire Oraison*, and in *Les Torrents*. A similar but less minute description would be found in Suzo and Ruysbroeck and in several of the Italian and Spanish Mystics.

yourself to his will. Self-surrender is the important thing in the Christian life; it is the key of the spiritual life. Who knows well how to surrender is on the speedy way to perfection. 'Whatever the soul does to support itself is an irreparable loss.'¹

But the Primitive Will does not yield readily. It is a long road, passing through many a slough of despond, that leads to complete resignation. For the soul begins by opposing with all her might the evil that is in her instead of putting herself in passivity. She falls and falls again; moans, repents and calls for help. But God seems not to hear. The time comes when she well nigh gives up hope; then she becomes indifferent, resigned to her fate whatever it may be. Thus is worked in her a condition of utter powerlessness so that, at last, through the Valley of the Shadow of Death, the soul enters into the surpassing peace destined for those who have become clay in the hands of the Divine Potter.

When the transformation is completed, 'the soul is no more her own, she is owned; she lives of the life of God. She *is* no more; God alone is. Formerly the soul felt that the 'Natural Man' wanted to take part in what was going on and duty consisted in restraining him; now she has taken the habit of keeping motionless. She lets herself be led, unconcernedly, neither thinking nor choosing.' The fusion of the Individual and of the General Will has taken place.

What a blessed state no longer to feel the jars of desires, no longer to hear the creakings of conflicting ideas! It means the disappearance of the forefront of ordinary self-consciousness, of the painful antagonisms and irritating obstructions by which we are chiefly brought to self-consciousness. 'The heart, says Mme Guyon, is now always free, easy, contented.' There reigns an abysmal peace, so deep as to put one on the verge of non-existence. And yet the moral nature sleeps not; it has by no means lost its fine edge. The obligatoriness and the supreme worth of God's Will are as clearly present as ever. If resisted, a conflagration flares up to be quelled only by a renewed self-surrender.

¹ Passages from *Le Moyen Court* abbreviated and brought together.

The creed of these people consists of two propositions. Slave of destiny, altogether impotent and predestined, is the crushing first statement. It is, however, clothed in blessed meaning by the second: the Ultimate Intention is supremely good; therefore fear not, you may trust and surrender yourself to the workings of Nature and it will realize in you its glorious end. They do not put it quite in these words, but it is after all the meaning of their experience. Their predestination doctrine is consonant with the most radical optimism.

Put in psychological terms their programme means the withdrawal of attention from oneself, trying to become unconscious to all that passes within, to the good and the bad alike. Does not this amount to an unmitigated denial of the efficiency of effort in the moral life, and, even beyond that, to a denial of the usefulness of consciousness, on its intellectual aspect at least? These men are wooers of the instinctive, of the sub-conscious. Their whole life is a protest against the glare of consciousness; they want that intellectual darkness in which the individual light is replaced by the undifferentiated effulgence of the divine spirit. This bombastic phrase corresponds in them to something very real, to an affective tendential consciousness well described by the two words love and righteousness which characterize God in the mind of the Christian Mystics. They do not really want absolute unconsciousness, but only the suppression of the *presentational*. They would retain only the *feeling* of God's love and the general *desire* to do His will.

There is an apparent contradiction between the denial of the value of effort and the life-long strivings of the Mystics to become passive, to give up. But we may readily pass beyond the words to a meaning not in the least contradictory. Their aim is to cease worrying, cease repining, cease despairing, cease looking in the face of evil to give it battle. It involves, indeed, if you choose to call it so, an effort never to be given up, but it is an *effort to relax*, instead of an effort to contract. They say, 'let go' instead of 'grip.' Do not surrender to evil, but circumvent it by not thinking of it. There is here a real difference of procedure; the ambiguity of terms should not be allowed to conceal it.

The Mystical Death appears to us as a functional anæ-

thesia, falling upon particular regions. In Mme. Guyon it is deeper than in most other great Mystics. She is a favored subject. An hysterical temperament confers upon her histrionic possibilities hardly within reach of more stolid natures. The anæsthesia is likely to bear with particular intensity upon certain bodily functions, those most completely disregarded. Mme. Guyon, for instance, declares that her love for chastity was so great that there is nothing she would not have done for it. Nevertheless she fulfilled her duty toward her husband, but she desires the reader to know that, as to that as well as to the other organic functions, her heart and her mind are so well separated from her body that *il fait les choses comme s'il ne les faisait pas*.

One may wonder why it is that the self-resignation method should lead to the results it is intended to secure—the weakening of the primitive man and the strengthening of the *Socius*,¹ and not rather to the reverse. *S'abandonner*, means, for one, muscle-relaxation, the disappearance of the mass of tensions affecting both the visceral and the voluntary muscles, tensions which are the necessary motor side of desire. Do away with them and the desire loses its forcefulness, its aggressiveness; it is transformed into a mere idea, and ceases to exist as desire. The tensions acquire a particular intensity whenever there is opposition. That is precisely the usual condition of the Mystic with regard to all desires and impulses in disagement with the Will of God. Passivity—return to quietude—involves the disappearance of these unwelcomed activities and of the painful tension-feelings accompanying them. The practice may then be looked upon as the application of the Gospel of Relaxation to the moral life. But if self-surrender gives its quietus to the Primitive Man why does it not also prevent the expression of God's Will? Why is it precisely when the 'Natural Man's' hungers have been suppressed that the Universalized Will appears in its greatest clearness? The difficulty vanishes as soon as the *cognition* of God's Will is distinguished from the desire and the endeavor to perform it. In the silence of appeased

¹I borrow this term from Prof. J. Mark Baldwin to designate that part of the individual which implicates the social relations, as, for instance, the tendency to the universalization of action.

strife the voice of conscience may ring out in unmistakable tones, *i. e.*, the Universalized Will is apprehended. So far, relaxation is advantageous. There remains, however, the execution, and here muscle relaxation would of course bring abortion. It is unfortunately the fault into which some of the most extravagant Mystics have fallen; their quietism has not been distinguishable from indiscriminating inertness. But this is only a misapplication of the surrender-remedy. Properly used, it induces internal peace, and, as consequences, a clearer perception of the Divine Will and a readier performance of it since its antagonists have left the way clear. The concordant relation existing between the self-surrender doctrine, the Lange-James theory of the emotions and "mind-cure" will appear to every one.

It hardly need be added, that the end sought, and more or less completely realized, by the Christian Mystics in the State of Death, is not necessarily bound up with that state. It is to be regarded as a device of a particular group of persons to give satisfaction to their dominant inner need. The degree of practical value and of reasonableness which belongs to it will probably appear clearly enough from the preceding pages.

The belief in the practical wisdom of absolute self-surrender is found in some form or other at the bottom of all the ethical religions, even in that of the Hebrews, the most stubbornly willful of all peoples. Primitive Buddhism and Islam are built upon the belief in the inefficiency of the human will and it is a dominant note in all forms of living Christianity. Were we to compare the practice of the Christian Mystics with that of representatives of primitive Buddhism, we should discover a fundamental agreement. Experience affirmed, there as here, that the mystical practice with regard to consciousness is the most efficient one in the moral life. Is it not also essentially the one which modern psychology would prescribe *under the same circumstances?*¹ In the State of Death, the mystics have produced a transformation of the individual which, whatever may be its practical value, is a serious attempt at a contribu-

¹On the usefulness of Meditation, Contemplation and Ecstasy, see my paper on Mysticism in the *Rev. Phil.*, 1902, XI, pp. 449 and ff.

tion to human development. It is an empirical solution of the salvation problem as different from that offered by Hartmann as a stage melodrama differs from real life. To deride this notable experiment under the pretext that it is the work of unbalanced brains would show ignorance and prejudice. Neither the road they have followed, nor the goal they have pursued is of their own discovery. The essential directions were already set down by Jesus and his disciples, not to go further back. The similar death to sin, and the new birth by which all things become new, are parts of Christian theology. Statements of like import are to be found in the sacred literature of other nations. The contribution of the Christian Mystic has been to bring to a logical conclusion a tendency constituting one of the dominant variation-forces in human society. Relentlessly, heroically, with the determination of fate, they have pushed to the radical solution where the rest of mankind were content with make-shifts and doubtful compromises. Theirs may not be a perfect solution; the range of their experience may have been too narrow; but as far as their own consciousness is concerned they have reached an absolute, a final, settlement. For the rest, we may well remind ourselves that, in the end all is relative, and allow each man to decide for himself whether that same solution is demanded by his own nature and whether it has for him the appearance of an ultimate goal.

It remains for us before bringing this paper to a close to consider briefly the bearing of the experience we have described upon the problem of development.

The life of the Mystics teaches, more forcibly than that of any group of men known to us, that if progress is conditioned, as we are assured, by fortuitous variations and also, possibly, by changes initiated by the individual will in its endeavor to adapt itself to the external world, there is still another source of variation properly called *inner adaptation*.

Taking up first the second of these possible sources, we say that there is satisfactory evidence that the transformation we have studied is not called for by the *milieu*, at least not as usually understood. The Christian Mystics are the prey of impulses which usually set them more or less completely in

antagonism to their surroundings. Those only who seek the seclusion of a religious community may find an atmosphere responsive to their inner needs. As for the others, for instance Mme. Guyon in the greater part of her life, they are the subjects of persistent and strenuous conflicts between their own inspiration, designated as God's Will, and the demands of the World. Instead of wishing to adapt themselves to society, they strive to live by principles which make of society their enemy. Avoided, persecuted, they nevertheless press onwards, urged by a martyr-making power. They neither reflect, nor follow, but endeavor to meet the requirements of a society without existence beyond their own mind. They are self-sufficient centers of life against which neither man nor devils can prevail. And in all this they are no more than types of the so-called 'spiritual' man who, in all ethical religions, as well as outside of them, but especially in the Christian faith, opposes himself more or less completely to the World, renounces it in part and, at times, refuses to bow down before it. In the light of these facts the utter crudity of the development formula 'struggle for life' is evident. Nothing intelligible has been said until the kind of life wanted has been described. As far as the tendency we are studying is concerned, life means the death, or at least the subjection, of some of the most fundamental impulses of primitive human nature.¹

In this connection and parenthetically, we may briefly summarize the evidence offered by the seekers after Mystical Death on the much debated relation existing between pleasure and action. The Mystics are no doubt convinced that the highest happiness would accompany the realization of the end they seek. It does not follow, however, that they are moved by anticipated pleasure. Pleasure shows itself to be, in their case, a very subordinate cause of action, if it can be said to be a cause at all. Whoever has tried to put himself in the attitude of the ascetic subjecting himself to year-long tortures understands that he is actuated by other forces than the love of affective satisfaction.

¹ The importance for animal evolution of the inner factor is being more and more fully recognized. Few have been as clearly aware of it as Prof. Irons. See his paper *Natural Selection in Ethics*, *Phil. Rev.*, May 1901, X, 3.

It would be the height of absurdity to say that tendencies so irresistible and so stupendously reckless as to the intensity and quantity of pain they involve, are under the sway of pleasure-pain. There is never any bargaining with hedonic values, but only duties to be done without regard to affective consequences. The principle to be accepted with all it implies may be formulated as follows: The nervous force which produces action is directly proportional neither to the intensity nor to the quality of the foreseen pleasure, neither is it proportional to the intensity of the desire or of the effort. In other terms, the power of action need not be, and usually is not, exactly represented in consciousness.¹

If, on the one hand, the tendency to the universalization of action with its consequences is not called for by the demands of society as it is, but is an inner adaptation, it is not, on the other, to be looked upon as fortuitous. It is, on the contrary, a logical, necessary outcome. A full discussion of this point would lead us far afield while the space at our disposal limits us to a few words.² By *logical* and *necessary* we mean just what would be meant in the case of the formation of general, abstract ideas from particular, concrete ones. The psycho-physiologist is not at a loss to indicate in a general way how, the structure of the human nervous system being given together with perceptive experiences, general or universalized ideas are necessarily formed coincidentally with new cerebral activities. The general idea can no more be said to be a fortuitous variation than the meeting of objects at the bottom of an inverted cone could be called a chance aggregation. Whatever chance there may be—if chance there is at all—is to be looked for beyond the data of our problem.

Is the inner adaptation with which we are concerned and the universalized activities it produces destined to survive? The doctrine of the survival of the fittest, as ordinarily in-

¹Taken with slight modifications from the *Revue Philosophique*, July, 1902, p. 25.

²The interested reader will find them supplemented in a paper, hasty in many respects, written several years ago: *The Psycho-physiology of the Moral Imperative*, *Amer. Jour. of Psy.*, 1897, Vol. VIII, pp. 528-559.

terpreted, would require a negative answer. But since a changing individual makes an altering society, what is impossible at one time may be fit at another. The tendency to universalized action is general at a certain level of development. Some regard for it and a certain degree of conformity to its promptings are already now requisites of what we are in the habit of regarding as civilized life, even though the world is still an unfriendly place for those who achieve the absolute surrender of everything clashing with the universalized will. As the tendency in question is a logical, necessary product, we may, it seems, confidently predict that the world will become more and more habitable to them because more and more transformed by them. One may even foresee the day when the triumph of the Universal Will in every human breast will have solved many of the difficulties now darkening the social horizon.

As to the question of heredity this only need be said here—if acquired characters are not inheritable, then, until a chance congenital variation brings about in its own blind way what particular individuals now achieve with much labor, the world will have to rest content with social heredity.

PRIMITIVE TASTE-WORDS.

By Professor ALEXANDER FRANCIS CHAMBERLAIN, Clark University.

Important for the student of comparative psychology, but as yet imperfectly investigated, are the taste-words of savage and barbarous peoples. From the field of the Algonkian languages, in which for a number of years he has made special research, the writer offers this brief contribution to the literature of the subject. The Algonkian is one of the most extensive stocks of North America, and the tribes belonging to it are typical American Indians, physically and mentally. The records we possess of their languages and dialects are of such a character as to enable us to speak with confidence concerning such sections of their vocabulary as we may desire to examine.

Among the Algonkian words of a more or less generic sort for "taste" are: Massachusetts *qutchtam*, "he tastes;" *qutchta-moonk*, "tasting, the sense of taste;" *spuhquodt*, "the taste or flavor of anything." Lenâpé *guttandamen*, "to taste;" *meschandamen*, "to taste;" *migopoquoak*, "taste." Ojibwa-Nipissing *ipogwad*, *ipogosi*, "it tastes;" *ipogosiwin*, "taste;" *nin godjipidjige*, "I taste;" *nin tangandan*, *nin tangama*, *nin gotandan*, *nin gotama*, "I taste it, try it." Cree *kutchistawew*, "he tastes." The Cree language uses generally in lieu of a special word for "taste" the radical-suffix—*spokusiw*, *spokwan*, "taste, having a taste." The Massachusetts *qutchtam*; Ojibwa-Nipissing *godjipidjige*, *gotama*; Cree *kutchistawew*; as Massachusetts *qutchehhew*, Cree *kutchihew*, "he tries, makes a trial of," indicate, are derived from the Algonkian radical *kut*, *got*, *godj*, *kodj*. To Cree *kut* Lacombe (Dict., p. 424) assigns the meanings "to try, make trial of, taste," which significations attach also to the Ojibwa-Nipissing *got*, *godj*, etc. Thus "to make trial of (by tasting)" seems to be the original significance of these taste-words, a sense not far from that of some of the corresponding terms in the Aryan languages. Massachu-

setts *spuhquodt* is cognate with Cree *-spokwan*, Abnaki *-pugwat*, Ojibwa-Nipissing *ipogwad*, etc., representing an Algonkian—*pokwat*,—"having a taste, being so-tasted." Lenâpé *migopoquoak* contains the same root. From the same source also is derived Ojibwa-Nipissing *nin godjipwa* "I seek to know the taste of it,"—in French *je le déguste*. The literal sense of Ojibwa *nin tangama* is "I touch with the mouth," from the radical *tang*, "to touch," and the radical-suffix *am* "mouth." Another general taste-word is seen in Ojibwa-Nipissing *nin nisitopidjige*, "I know it by the taste," "I recognize the taste,"—the first radical is *nisit*, "knowledge."

A distinction between "good tasting" and "bad tasting" is general among the Algonkian tribes considered here: Abnaki *uripugwat*, "good tasted," *matsipugwat*, "bad tasted." Ojibwa *minopogwad*, *minopogosi*, "it has a good taste;" *mangipogwad*, *mangipogosi*, "it has a bad taste." Cree *miyospokwan*, *miyospokusiw*, "it has a good taste;" *matchipokwan*, *matchipokusiw*, "it has a bad taste." Lenâpé *wulipoquot*, "it has a fine taste," *machtschipoquot*, "it tastes ill." In all of these words the prefixes *uri*, *mino*, *miyo*, *wuli*, signify "good," and *matsi*, *mangi*, *matchi*, etc., "bad," being the ordinary terms for those ideas. The radical-prefixes *wishko*, "sweet," *wisa*, "bitter, disagreeable," *wingi*, "excellent," etc., are in like use. To the Nipissing radical *wishago* Cuoq (Lex. alg., p. 435) assigns the meanings, "disagreeable to the taste or to the smell." From it are derived *wishagopogwat*, "bad-tasting, disagreeable to the taste," and *wishagomagos*, "bad-smelling, disagreeable to the smell." From the same radical comes *wishagosi*, "he is bad-odored,"—said of a person who has eaten garlic, or one who is hairy and sweats much. In Lenâpé we find *niskandamen*, "to taste nasty," with the radical-prefix *nisk*, "nasty, dirty," and the verbal suffix-radical "to taste (with mouth)." Another interesting taste-word of a rather general sort is found in Cree *ayisipiwokisiw*, "it has no taste but that of water,"—cf. *ayisipiy*, "clear broth, nothing but water."

Among words for "insipid," "tasteless," etc., may be cited Objibwa-Nipissing *pinisipogwad*, *pinisipogosi*; Cree *pihisipokusiw*, *pihisipokwan*, "it is insipid," said particularly of victuals. Lacombe has also (Dict., p. 575) *piyekusiw*, "it is insipid,

dry, tasteless" (*e. g.* meat). The first words seem to signify literally "devoid of taste."

Following are some of the special taste-words in certain Algonkian tongues:

Acid. For "acid" Lacombe gives in *siwittin*, "it is acid," also *siwisiw*, "it is acid, sweet, salt," from the radical *siw*, "acid, sweet, salt, sour, sharp." Cuq has in Nipissing *shi-wan*, "it is acid." For "acid, tart," Rand has in Micmac *sāookw*, properly the word for "sour." The Algonkian languages here considered express "sour" and "acid" by words derived from the same root. See: *Sour*.

Astringent. Lenâpé *tiechtpan*, "bitter, astringent, puckery," seen in *tiechtpanihm*, "white hickory nut" and cognate, perhaps, with Menomini *titaqbiu*, "bitter."

Bitter. Massachusetts *wesogkon*, Lenâpé *wisachgan*, Micmac *wěskūk*, Ojibwa-Nipissing *wisakan*, "it is bitter," contain the radical *wisak*. To this radical Cuq (Lex. Alg., 1886, p. 442) assigns in Nipissing the meanings: "Amer, piquant, douloureux, en souffrance, cuisant, brûlant, à demi brûlé, vif, éclatant," and Lacombe (Dict., 1874, p. 653): "To suffer, to feel pain, bitter." Trumbull (Natick Dict., p. 186) seeks to connect Massachusetts *wesogkon*, "bitter," with *weeswe*, "gall," whence also perhaps *weesoe*, "yellow" (*cf.* English yellow—gall)—the cognate words for "gall" in other dialects are Cree *wisopiŷ*, Ojibwa-Nipissing *winsop*, Menomini *wesup*, Micmac *wiskūm*, etc. This connection is, however, not certain. The wide range of significance of the root *wisak* can be seen from the following list of derivative words: Lenâpé *wisachgissi*, "it hurts;" *wisachgank*, "rum, brandy" (so-called from its sharp, biting taste); *wisachgin*, "wild grapes;" *wisachgak* "black oak" (literally "bitter wood"). Micmac, *wiskōk* "black ash." Ojibwa-Nipissing *wisakibak*, "the leaf is bitter;" *wisagak*, "a species of ash-tree" (*frêne gras* of Canadian French); *wisagisi*, "it (bread) is bitter;" *wisagagami*, "it (liquid) is bitter;" *wisakashkate*, "he has the stomach-ache;" *wisakishtikwan*, "he has the head-ache;" *wisakakis*, "he suffers from a burn;" *wisakate*, "it is very hot in the sun;" *wisakwe*, "he has a harsh voice;" *wisakande*, "it (dress) is of a loud color." Cree *wisakimin*, "cranberry" (literally "bitter

berry"); *wisakâgamîw*, "it is a bitter liquid;" *wisakâbiw*, "he has pain in his eyes;" *wisakasew*, "his skin is sensitive (hurts);" *wisakiskâkuw*, "he has indigestion;" *wisakitchew*, "he has heart trouble." The radical *wisak* thus comprehends, in these Algonkian languages, the ideas of "bitter" in our sense of the term, "pain and suffering," the sensation of "burning," the "sharpness" of pains and feelings, the "heat" of the weather, the "harshness" of the voice, the "loudness" of a color, the "sensitivity" of the skin, etc. It includes also the "bitter" tastes of animals, plants, minerals, liquids and other substances.

Another Algonkian word for "bitter" is Cree *âkusiw*, *âkwan*, "it is bitter," from the radical *âk*, to which Lacombe (Dict., p. 288) assigns the meanings "amertume, âpreté, douleur, méchanceté." From this root are derived: *âkkoheiw*, "he makes him suffer great pain;" *âkwâtisiw*, "he is cruel;" *âkotonâmw*, "he has a ferocious tone of voice;" *âkwâstew*, "it is very hot;" *âkwatchiw*, "it is frozen, hardened by the cold;" *âkwâkatosuw*, "it is hardened by drying;" *âkkusiw*, "he is sick." The corresponding radical in Nipissing is *ako*, defined by Cuoq (Lex. alg., p. 33) as "mal, fort, rude, mauvais, désagréable." From this root are derived the Nipissing words: *akotagos*, "to have a harsh tone of voice;" *akomagos*, "to smell strong, to have a bad odor;" *akoshkate*, "to have the colic;" *akwagami*, "bitter, sharp, piquant liquor."

Peppermint. The Ojibwa word for "peppermint," *tekassing*, signifies, literally, "something, cool or cooling," from the Algonkian radical *tak(a)* "cold, cool." The cold "feel" of peppermint is here with the Indian the pronounced sensation, as is the case with many Europeans, especially children. The stinging, "hot" sensation seems to be subordinate. The taste of peppermint is submerged, apparently, in the "coolness."

Pungent. Several of the Algonkian dialects have borrowed their word for "pepper" from the French of North America,—Ojibwa-Nipissing *dîpweban* or *tîpweban* is a corruption or remodeling of *du poivre* (in Canadian French *du pwevre*). Ojibwa has, however, also the term *(ga)wisakang*, Mississauga and Nipissing the simpler *wisakan*, literally "it is bitter, sharp," from the radical *wisak* discussed under *Bitter*. Micmac *dâpesawââl*,

"pepper" seems likewise to be of foreign origin and not a native word made to occasion. The Cree language, in which "pepper" has been named from the form of the "corns," uses for "to season with pepper" *siwinew*, a derivative of the radical *siw*, "acid, sour, sharp." Ojibwa has escaped naming "mustard" from its taste by calling it *wesâwag degwandaming*, "the yellow thing that is eaten with other things." Micmac has *lamûltald*, borrowed from French *la moutarde*.

Rancid. Ojibwa-Nipissing *satepogwad*, *satepogosi*; Cree *sâstesiw*, *sâstesin*, "it tastes rancid," contain the Algonkian radical *sate*, *sâste*, which Cuoq defines in Nipissing as signifying "rancid," and Lacombe in Cree as "rancid, bad-tasting, partly spoiled, bitter, insipid." It is worth noting that in Nipissing *satewe*, from the same root, means "he has a hoarse voice."

Salt. Many (perhaps most) of the Algonkian tribes were unacquainted with salt until after their contact with the white man. Concerning Eliot's famous translation of the Bible into the language of the Massachusetts Indians Trumbull says (Natick Dict., 1903, p. 317): "The English word is transferred by Eliot, the Indians not having then learned the use of salt. In a single instance, 'salt water' (James, 3, 12) is rendered *seippog*, i. e. 'sour water'." The Micmac term for "salt" is *salawa* (from French *sel*), upon which word Rand thus comments (Micmac Dict., 1888, p. 224): "Here is evidence that the Indians used no salt before they obtained it from the whites, since they had no name for the article." The Menomini, according to Hoffman (14th Ann. Rep. Bur. Ethnol., p. 286): do not use salt: "Salt is not used by the Menomini during meals, neither does it appear to have a place in their kitchen for cooking or baking. Maple sirup is used instead, and it is singular how one may acquire the taste for this substitute for salt, even on meats." Among Algonkian words for "salt" are: Ojibwa-Nipissing *shiwitagan*, Cree *siwittâgan*, Menomini *shéweqtaken*, derived from the root *shiw*, *siw*, "acid, sour," further discussed under *Sour*. Hoffman says that the Menomini word for "salt" signifies "sour and sweet,"—evidence of the mixed character of the Indian's reaction to the taste-stimulation of salt. The Blackfoot term for "salt," *istsexipoko*, is derived from *istsipoko*, "bitter." As Lenâpé words for "salt"

are given (Brinton and Anthony, Dict., 1889, p. 132, p. 131), *sikey* and *schewunk*,—also *schuon*, “saltish, sour;” *schwerwak*, “salt meat;” *sikeyhasu*, “salted, pickled.” The second of these Lenâpé words is evidently from the Algonkian radical *shiw*. The first, *sikey* is probably from the radical *siki*, *shiki*, “urine,” with reference to the taste. *Schwon* is identical with Ojibwa *shiwan*, “it is sour.”

Some of the southeastern Algonkian peoples, of Virginia and the region westward, where “salt licks” occur, knew salt, which formed one of the articles of their primitive commerce, as Mr. W. W. Tooker has shown. (The town-name *Mahoning*, from Lenâpé *mahonink*, “at the ‘deer-lick’ ” preserves one of the Algonkian terms for these places). The Chaouanons were so called because they were “salt-makers” (Amer. Antiq., Jan., 1895). As the names for “salt” employed by the Virginian Algonkian people prove they also called it after the “sour” taste, as did the northern tribes cited above.

Sour. A widespread Algonkian radical for “sour, acid” is *siw*, *shiw*, seen in Massachusetts *sée*, Ojibwa-Nipissing *shiwan*, Cree *siwaw*, Lenâpé *sh’won*, etc., “it is sour.” From this radical have been formed the following words: Massachusetts *sée petukqunnunk*, “unleavened bread;” *séog*, “what is sour;” *nukkone séog*, “leaven;” *séane*, “sour (unripe fruit)” and “sour” (of drink); *seïppog*, “salt water” (literally “sour water”) Ojibwa-Nipissing *shiwabik*, “alum” (literally “sour stone”); *shiwibak*, “sorrel” (literally “sour leaf”); *shiwitagan*, “salt” (literally “sour thing”); *shiwitiganabo*, “brine, pickle” (literally “salt liquid”); *shiwagamisigan*, “sirup” (literally “sour or sweet (?) sugar drink”); *shiwan*, “acid, tart” (of berries); *shiwagamisin*, “it (milk) is sour;” *shiwab*, “his eyes cannot bear the light;” *shiwas*, “he is dazzled” (*i. e.* by the “sharpness” of the light); *shiwabo*, “vinegar” (literally “sour liquid”). Cree *siwattāgan*, “salt;” *siwattāganābuiy*, “brine, pickle;” *siwābuiy*, “vinegar;” *siwapak*, “rhubarb” (literally “sour leaf”); *siwatew*, “he feels sour at the stomach” (from emptiness); *siwisiw*, “it is acid, sweet, salt;” *siwāgamiw*, “the liquid is sweet, salt, etc.,” *siwittin*, “it is acid;” *siwāsuw*, “the sun hurts his eyes.” To the Nipissing radical *shiw* Cuoq (Lex. alg., p. 92) assigns the meanings “acide, aigre, âpre,

sûr, salé," and Lacombe (Dict., p. 599) for the Cree *siw* gives "acide, sucré, salé, aigre." The Algonkian radical *siw*, *shiw* thus includes the senses of "sour" (as in minerals, plants, fruits, liquids, etc.), "acid," "salt," "sweet (sugared)," "effect of light on the eyes," (by reason of its "sharpness"), "sourness at stomach," etc. The comprehension of "sour," "acid," "salt" and "sweet" under one root is noteworthy. In Menomini, another Algonkian dialect the words for "sweet," *shéwan*; "sour," *shéwegnen*; "salt," *shéweqtâken*; and "sirup," *shéwakamitâ*, all contain the same root *shew*, *shiw*, *siw*. The designation of "sirup," Ojibwa-Nipissing *shiwagamisigan*, (literally "sour or sweet (?) sugar drink") by the radical denoting also "acid" and "salt" is interesting in connection with what Dr. Hoffman has said about the use of maple sirup with meat, etc., by the Menomini as a substitute for salt.

Sweet. One Algonkian radical for "sweet" is seen in Massachusetts *weekon*, "it is sweet;" Lenâpé *wingan*, "sweet, savory, good-tasted;" Micmac *wikw*, "sweet;" Cree *wikkasin*, *wikitisiw*, "it is sweet to the taste;" Ojibwa *wingipogwad*, *wingipogosi*, "it has an excellent taste." To the radical *wikk* in Cree Lacombe (Dict., p. 649) assigns the meanings "agréable au goût, à l'odorat, aimable," and for Nipissing *wing* Cuoq (Lex. alg., p. 440) gives "agréable, doux, bon, très-bon, excellent." From the Algonkian radical *wik*, *wing* have been formed among others the following words: Massachusetts *weekontamunk*, "pleasure, gladness, joy, delight;" *weekontamunat*, "to be glad, to rejoice;" *wekontam*, "he is glad;" *weekontamwal*, "glad, joyful, merry." Lenâpé *wingandamen*, "it tastes good;" *wingapue*, "good, sweet broth;" *wingel*, "eatable;" *wingi*, "fain, willingly, gladly;" *wingimachtek*, "odoriferous;" *wingimacquot*, "it has a good, pleasant smell;" *winginamen*, "to delight in, to be pleased with." Ojibwa-Nipissing *wingashk*, "aromatic plant;" *wingagami*, "it is an excellent, good-tasting liquid;" *wingawis*, "it (animal) is gentle." Cree *wikask*, "aromatic plant;" *wikkihew*, "he likes it;" *wikkimamew*, "he finds it agreeable to the smell;" *wikkimâkuhun*, "perfume, aromatic substance;" *wikkimâsum*, "it is odoriferous;" *wikkimâswew*, "he burns incense." The Algonkian radical *win*, "to have an odor, a smell," seen in Lenâpé *winak*,

"sassafrass," belongs here also perhaps. The Algonkian radical *wig*, *wing* has evidently comprehended a variety of significations, "good," "pleasant," (particularly to the senses of taste and smell), "aromatic," "odoriferous," "sweet," etc. The same confusion of "taste" and "smell" is found in the Aryan languages. The primitive meaning seems to have been "agreeable or pleasant" (to taste, smell).

Another term for "sweet" is found in Ojibwa-Nipissing *wishkobad*, *wishkobise*, "it is sweet;" and the derivatives *wishkobimin*, "sweet corn;" *wishkobagami*, "the liquid is sweet," etc. To the radical *wishkob* Cuoq (Lex. alg., p. 434) assigns the meaning of "succulent."

This brief study contains the chief facts concerning the taste-words of several Algonkian peoples, and brings out the primitive confusions and associations of the various senses naturally to be expected at the stages of culture considered. The reactions of the American aborigines to the stimuli offered them by the intruding Aryans, as expressed in the new terms of their vocabulary and the new twists given old words form the results of an unconscious psycho-physical experiment on a grand scale, which cannot fail to be of supreme interest to all students of mankind.

ON TIME JUDGMENT.

By BEATRICE EDGELL, M. A., Ph. D.

Communicated by Professor AUGUSTUS WALLER, University of London.

(From the Physiological Laboratory of the University of London.)

§ 1. THE QUESTIONS STUDIED.

The following experiments are concerned with two questions regarding Time Judgment.

I. What "filled" period of Time can be most accurately estimated?

II. When two filled periods of different duration are given is the duration of the period which is estimated as midway between them, the arithmetic or the geometric mean of the two periods?

The experiments dealt only with "filled" periods, *i. e.*, periods coloured by a continuous sensation of specific quality, these being in the opinion of the writer psychologically simpler than the so-called "empty" periods where there is no definite colouring, but a mingling of fleeting images and passing thoughts with organic and muscular sensations. Whatever be the data upon which Time Judgment is based, homogeneity in the character of the period would seem to favour estimation. The "filling" selected was that of sound. As grounds for the selection, two points may be noticed. (1) It is fairly easy to produce a period of sound which is homogeneous in character. (2) Sound has the advantage over Colour or Brightness of having sharper termini *a quo* and *ad quem* and is freer from after-image effects.

As far as the writer is aware the first question has not been made the special subject of investigation. Meumann's investigation (*Phil. Studien*, Bd. 12, 2 H.) which deals with the estimation of empty in comparison with "filled" periods and the influence of various kinds of filling upon the estimation, is closely allied to it. Schumann (*Zeitschrift f. Psy. u. Phys. der Sinnesorgane*, Bd. 4) has dealt with the question for

empty intervals. The second question is included in an investigation carried out by F. S. Wrinch ("Ueber das Verhältniss der ebenmerklichen zu den übermerklichen Unterschieden im Gebiet des Zeitsinns," Wundt's *Phil. Studien*, 18 Bd. 2 H). Although that investigation has a different purpose, a wider scope, and a far more elaborate method than that of the present series of experiments, yet in so far as it treats of the same question, it shows general agreement with the results given below.

§ 2. METHOD OF EXPERIMENT AND ARRANGEMENT OF APPARATUS.

The experiments were carried out in the Physiological Laboratory of the London University, continuing from May, 1902, until March, 1903.

The method of experiment used was the method of Average Error or, more descriptively, the method of Reproduction.

In Investigation I, the period which could be most accurately estimated was determined by the period which the subject could most accurately reproduce after its delivery.

Similarly in Investigation II, the subject's estimation of the mean between two given periods was determined by the period which the subject produces as his judgment of the mean period.

To carry out this method the following arrangement of apparatus was made. (See Fig. 1, p. 156.)

The subject of the experiment is placed in a room (Room 2) away from the recording apparatus and other distractions, and receives a sound, produced by induction currents at 50 per sec. in Room 1, by means of a telephone, in Room 2, attached to the secondary coil.

The duration of this sound is registered in Room 1 upon a kymograph by means of a Pfeil's signal inserted in the primary circuit.

This circuit can be completed by two paths. (1) It is completed automatically by the trailing of a spring (k_1) against a wheel bearing contact strips which is fixed on the rotating axis of the kymograph.

(2) It can be completed by a key in the hands of the subject, (k_2).

At every rotation of the contact wheel, the subject receives a sound of given duration regulated by the length of the con-

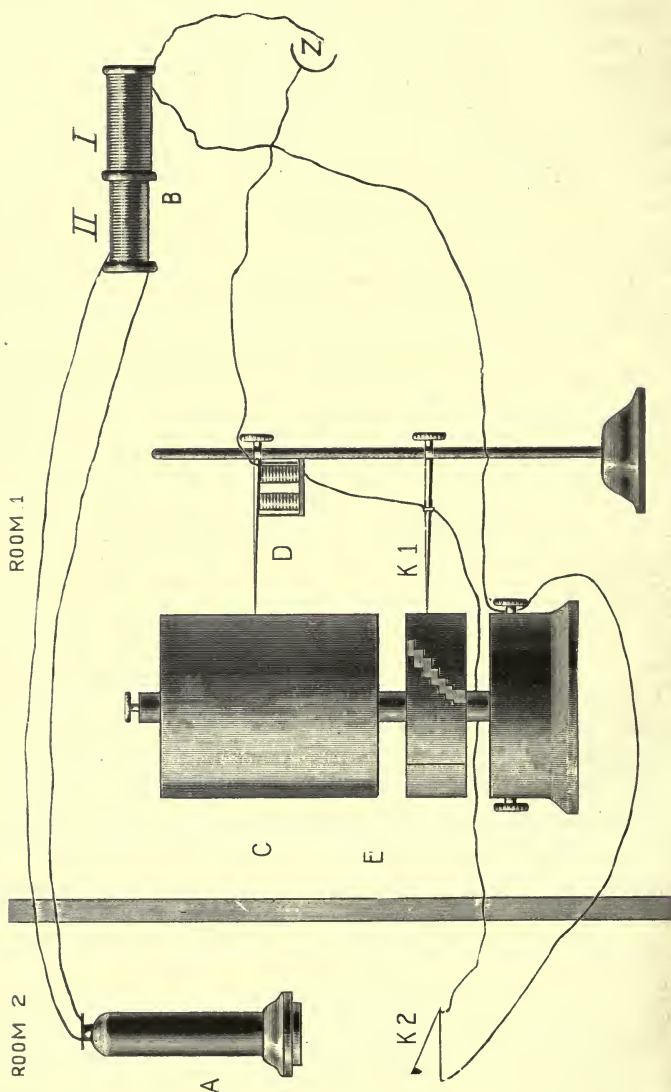


FIG. I.

tact strip to which the spring is adjusted and the rate of rotation. By closing his own key the subject reproduces in the

telephone a period of sound which seems to him equal in duration to that just delivered.

Both the standard, or 'Question,' duration and its reproduction, or 'Answer,' are registered by the magnetic writer upon the drum of the kymograph.

Throughout any one series the standard period is usually kept constant, variations being only introduced now and again to test the subject's attention; but the series themselves follow no regular gradation, long periods being given after short ones or *vice versa*.

In experiments to ascertain the mean, the subject receives two periods of sound, the strips upon the contact wheel being arranged in pairs. No regular order is followed in the two delivered periods. In one series the two periods may be in the order short-long, in the next series in the reverse order, long-short, if the subjects of the experiment have been tested for any constant error due to order.

The figures below (Figs. 2 and 3) give reproductions of two records.

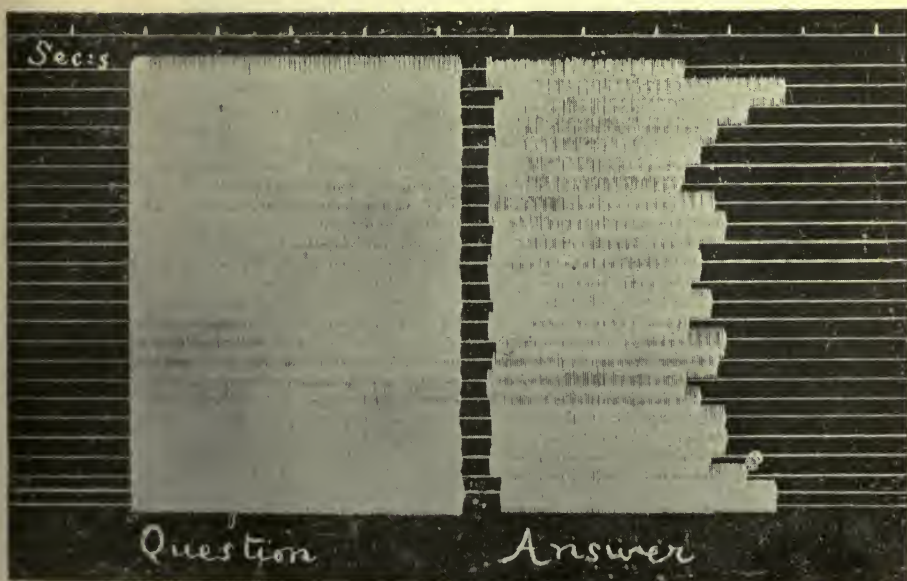


Fig. 2. Series of experiments wherein the subject sought to reproduce a period equal to the period delivered. (Subject S. Table I.)

PROTOCOL FOR FIG. 2.

Question		Answer	M.Variation	
34 mm. = 3502 σ	1	36.25	6.81	<i>Rate of Rotation</i> 9.75 mm. = 1 Sec. 1 mm. = 103 σ
	2	36.5	7.06	
	3	29.25	.19	
	4	31	1.56	
	5	30.5	1.06	
	6	27.5	1.94	
	7	26	3.44	
	8	29.75	.31	
	9	30	.56	
	10	26.25	2.19	
	11	28.75	.69	<i>Mean Estimation</i> 29.44 mm. = 3032 σ
	12	26.25	3.19	
	13	28	1.44	
	14	27	2.44	
	15	30.5	1.06	
	16	29.5	.06	<i>Mean Variation</i> 2.61 mm. = 269 σ
	17	25	4.44	
	18	25.75	3.69	
	19	28	1.44	
	20	29.25	.19	
	21	33.5	4.06	<i>Error.</i> —470 σ
	22	37	7.56	
	23	25.75	3.69	
Total		677.25	60.07	
$\div 23$		29.44	2.61	

The kymograph used in my experiments was that by Sandström (Lund) which is driven by an electric motor in the base of the instrument. Attached to the motor is a regulator by which the rate of rotation can be varied from .05 mm. to 1000 mm. per sec. Throughout the whole period of experiment the instrument ran with the greatest smoothness and accuracy, giving whenever tested the most satisfactory results. When tested half way through the whole term of experiment, the drum showed a mean variation of 16 σ per sec. at the 50 mm. per sec. rate of rotation, the rate used in the majority of the experiments.

Below (p. 160 [424]) is a cutting of a tracing given with Jacquet's Time Marker at the two speeds used, viz. 50 mm. per sec. and 10 mm. per sec. The straightness of the align-

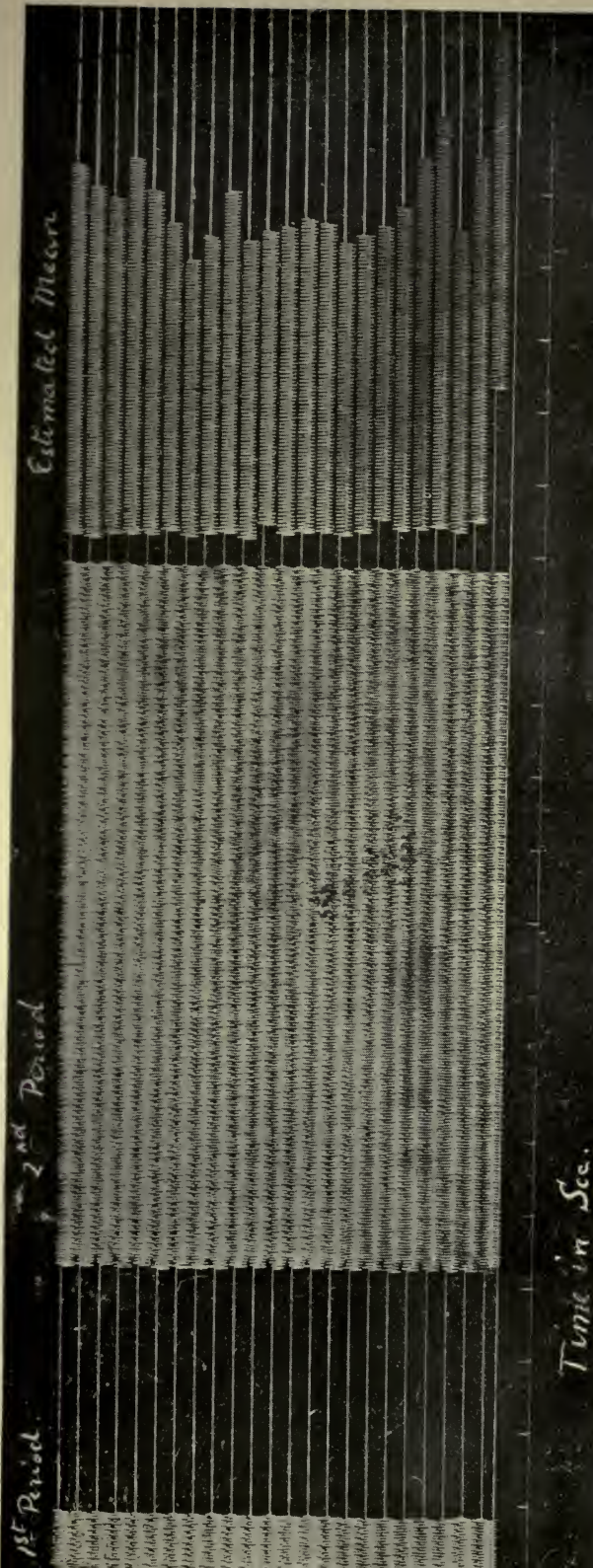


Fig. 3. Series of Experiments wherein the subject sought to produce the mean of the two delivered periods. Subject S. Tables II (c).



PROTOCOL FOR FIG. 3.

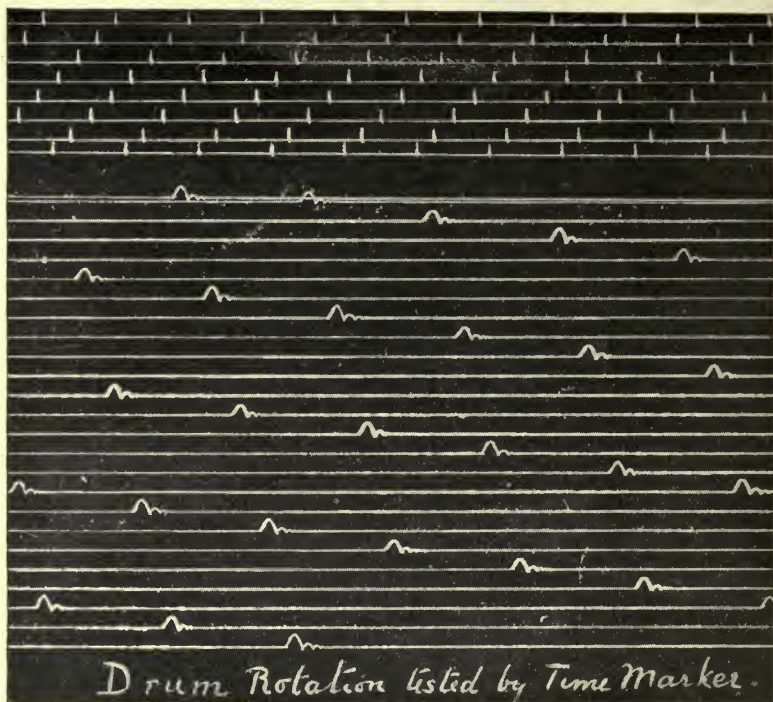
Periods given		Estimated Mean	M. Variation	
8.5 mm.	1	48.75	4.45	
and	2	49.25	4.95	
94 m.	3	40.25	4.05	<i>Rate of Rotation</i>
= 875 σ	4	56	11.7	9.75 mm. = 1 Sec.
and	5	50	5.7	1 mm. = 103 σ
9682 σ	6	43.5	.8	
	7	49.5	5.2	
	8	40	4.3	<i>Average Estimated</i>
	9	39.5	4.8	<i>Mean of given Periods</i>
	10	41.75	2.55	44.3 mm.
	11	42.25	2.05	= 4563 σ
	12	41.5	2.8	
	13	39.75	4.55	<i>Mean Variation</i>
	14	50	5.7	4.17 mm.
	15	45.75	1.45	= 429 σ
	16	39.75	4.55	
	17	37	7.3	
	18	41.25	3.05	<i>Arithmetic Mean</i> 5278 σ
	19	41	3.3	<i>Geometric Mean</i> 2911 σ
	20	40.5	3.8	<i>Error for A. M.</i> —715
	21	45	.7	" " G. M. + 1642
	22	47	2.7	
	23	49.75	5.45	
Total		1019.	95.9	
$\div 23$		44.3	4.17	

ment of time-marks on the successive lines of revolution of the cylinder (which descends on a spiral screw) affords a very exacting test of constant speed of revolution. The records of August 1st, at the outset of a series of experiments, and of Oct. 18th, at the close of a series, are practically indistinguishable.

§ 3. RESULTS.

I. In the first investigation three persons, S., B., and Sp., acted as subjects for systematic series of experiments. In the case of subject B., owing to the impossibility of further attendance, the series is less complete than is desirable.

There was no agreement shown in the period found favourable to estimation, this being 3.33 sec., 1.94 sec. and 1.07 sec. for the three subjects S., B. and Sp. respectively.



For each subject, however, it was found that periods longer than the favourable period were *underestimated*, those shorter than the favourable period were *overestimated*.

The details of the results are best shown by the following tables and curves.

TABLE I, FOR SUBJECT S. *Value in σ .*

Q.	A.	Error.	M. V.	M. V. %
255	594	+339	139	23.4
459	750	+291	91	12.13
673	913	+240	55	6
857	1122	+265	112	10
918	1234	+316	100	8.1
1040	1339	+299	128	9.56
1265	1462	+197	68	4.6
1390	1730	+340	251	14.5
1520	1742	+222	99	5.7
1673	1887	+214	141	7.5
1714	1964	+250	109	5.5
1918	2103	+185	71	3.35
2472	2650	+178	167	6.3
3348	3354	+ 6	212	6.3
3502	3032	-470	264	13.4
4532	3430	-1100	608	17.7

Q. = Period delivered. A. = Subject's estimation of the same.
M. V. = Mean Variation. Total number of experiments, 290.

Value in σ .

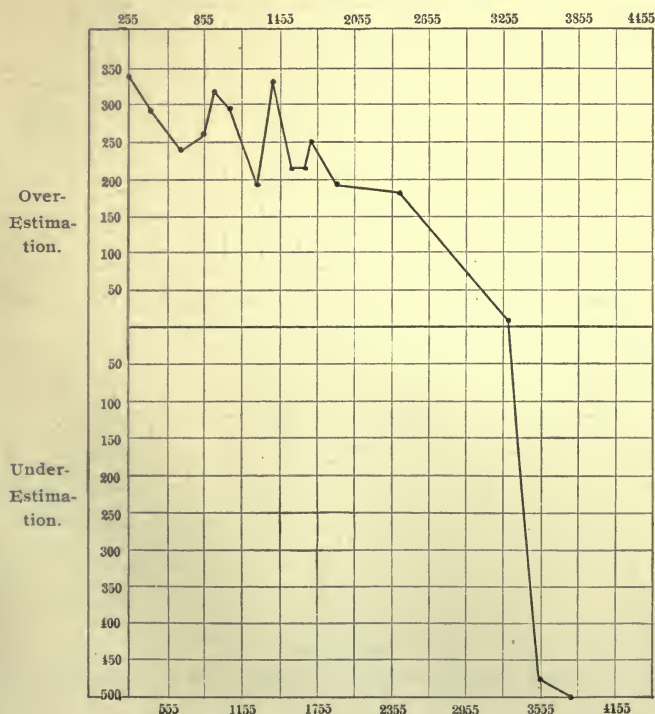


FIG. 4. Curve of Error for Subject S. from the above table.

TABLE I, FOR SUBJECT B. *Value in σ .*

Q.	A.	Error.	M. V.	M. V. %
257	319	+ 62	159	62.25
340	509	+169	71	20.88
515	653	+138	71	13.78
649	801	+152	87	13.4
659	853	+194	108	16.38
886	1154	+268	89	10.04
1000	1491	+491	123	12.3
1071	1281	+210	75	7
1257	1469	+212	116	5.42
1586	1700	+114	200	12.61
1710	1973	+263	202	11.81
1963	1949	- 14	121	6.16
3863	3707	-156	102	2.64
6522	6233	-289	521	8

Q. = Period delivered. A. = Subject's estimation of the same.
M. V. = Mean Variation. Total number of experiments, 444.

Value in σ .

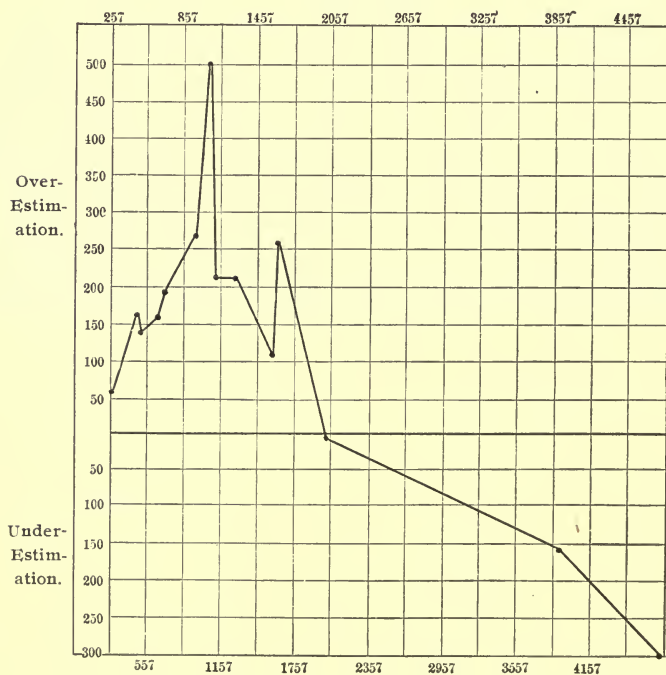


FIG. 5. Curve of Error for Subject B. from the above table.

TABLE I, FOR SUBJECT SP. *Value in σ .*

Q.	A.	Error.	M. V.	M. V. %
268	389	+121	45	17
474	518	+ 44	41	8.65
680	679	- 1	39	5.73
700	746	+ 46	43	6.14
886	876	- 10	90	10.15
1071	1081	+ 10	80	7.46
1318	1251	- 67	74	5.61
1463	1396	- 67	102	7
1931	1686	-245	67	3.46
2369	2206	-163	91	3.84
2421	2367	- 54	126	5.2
3502	3340	-162	116	3.31
4528	4356	-172	275	6.07

Q. = Period delivered. A. = Subject's estimation of the same.

M. V. = Mean Variation. Total number of experiments, 423.

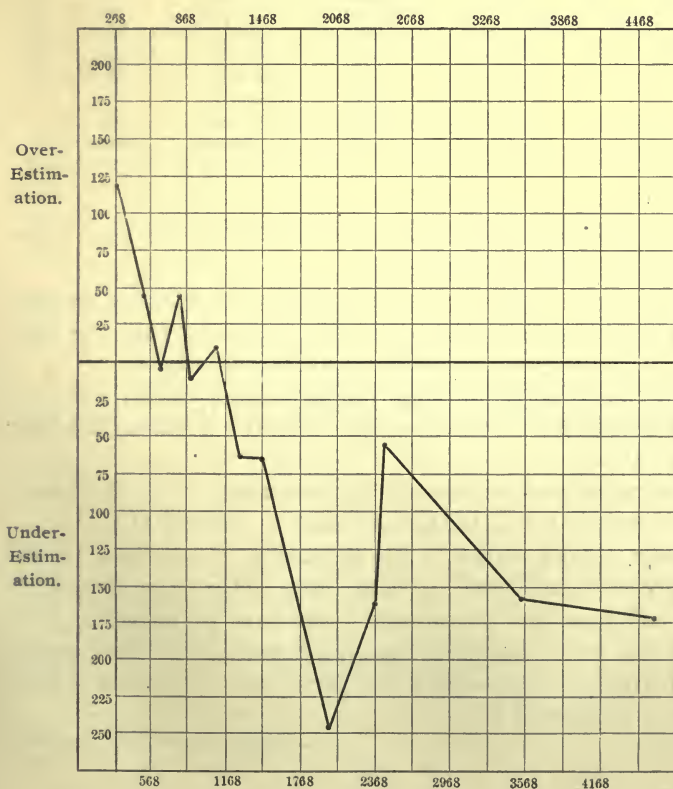
Value in σ .

FIG. 6. Curve of Error for Subject Sp. from the above table.

Besides the three systematic series given, single sittings of first trial experiments were taken with the subjects W., G., Wy. and R.

The results of these tend to confirm the general rule quoted above, showing *overestimation* in the case of short periods and *underestimation* in the case of long.

TABLE I, FOR FIRST TRIAL EXPERIMENTS ON SUBJECTS
W., G., WY. AND R.

Subject	Q.	A.	E.	M. V.
W.	886	1290	+404	107
	1936	2194	+258	135
G.	494	566	+ 72	45
	700	740	+ 40	41
	1061	1097	+ 36	75
	2369	2402	+ 33	223
	3450	2910	-540	772
Wy.	906	1080	+174	100
	1926	2075	+149	154
	3399	3430	+ 31	289
R.	711	849	+138	103
	896	954	+ 58	113
	3450	2831	-619	344

II. In the second investigation systematic series were made with the two subjects S. and Sp.

Speaking generally, the period judged to be midway between the two given periods was nearer the Arithmetic than the Geometric Mean, a result in conflict with Weber's Law.

With short periods, having, roughly, ratios from 1:8 to 1:6, the Estimated Mean is even greater than the Arithmetic Mean. (Tables II a.)

With longer periods, having the same ratios, the Estimated Mean is less than the Arithmetic Mean. (Tables II c.)

Greater approximation to the Geometric Mean than to the Arithmetic Mean occurs in those cases where the Geometric Mean falls near the "favourable" periods of Investigation I, or where the ratios between the given periods are very small. In experiments with subject Sp. these two conditions fell together. (Tables II b.)

TABLES II, FOR SUBJECT S.

- (a) Periods given short with large Ratio.
Estimated Mean greater than Arithmetic Mean.

Value in σ .

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{7.1}$	$\left. \begin{array}{l} 265 \\ 1897 \end{array} \right\}$	1081	709	1600	+519	+891	213	13.3
$\frac{1}{7}$	$\left. \begin{array}{l} 265 \\ 1877 \end{array} \right\}$	1071	705	1259	+118	+554	113	9
$\frac{1}{6.7}$	$\left. \begin{array}{l} 286 \\ 1938 \end{array} \right\}$	1112	744	1943	+831	+1199	175	9

R.=Ratio. A. M.=Arithmetic Mean. G. M.=Geometric Mean.

E. M.=Estimated Mean. M. V.=Mean Variation.

M. V. %=Reckoned on the Estimated Mean.

Number of Experiments, 284.

- (b) Periods given short with small Ratio.

Estimated mean less than Arithmetic Mean. In *two* cases nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{3.7}$	$\left. \begin{array}{l} 473 \\ 1751 \end{array} \right\}$	1112	910	1551	+439	+641	111	7.15
$\frac{1}{3.6}$	$\left. \begin{array}{l} 469 \\ 1693 \end{array} \right\}$	1081	891	1530	+449	+639	215	14
$\frac{1}{3}$	$\left. \begin{array}{l} 565 \\ 1709 \end{array} \right\}$	1137	983	1102	- 35	+119	101	9.15
$\frac{1}{1.5}$	$\left. \begin{array}{l} 844 \\ 1318 \end{array} \right\}$	1081	1055	934	-147	-121	184	19.7
$\frac{1}{1.3}$	$\left. \begin{array}{l} 927 \\ 1277 \end{array} \right\}$	1102	1088	1017	- 85	+ 71	131	13

Number of Experiments, 123.

(c) Periods given long with large Ratio.

Estimated Mean less than Arithmetic Mean. In *three* cases nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{11}$	875 } 9682 }	5278	2911	4563	- 715	+1652	429	9.4
$\frac{1}{8.5}$	1133 } 9682 }	5407	3312	3770	-1637	+ 358	268	7
$\frac{1}{7}$	1339 } 9579 }	5459	3581	4424	-1035	+ 843	380	8.6
$\frac{1}{6.5}$	1442 } 9476 }	5459	3697	3203	-2256	- 494	340	10.6

Number of Experiments, 76.

TABLES II, FOR SUBJECT SP.

(a) Periods given short with large Ratio.

Estimated Mean greater than Arithmetic Mean.

Value in σ .

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{8.6}$	227 } 1957 }	1092	667	1175	+83	+508	74	6.3
$\frac{1}{7.2}$	268 } 1936 }	1102	720	1170	+68	+450	102	8.6
$\frac{1}{6.7}$	144 } 968 }	556	373	648	+92	+275	94	14.5
$\frac{1}{6.2}$	309 } 1936 }	1123	774	1151	+28	+377	97	8.4

R.=Ratio. A. M.=Arithmetic Mean. G. M.=Geometric Mean.

E. M.=Estimated Mean. M. V.=Mean Variation.

M. V. %=Reckoned upon Estimated Mean.

Number of Experiments, 113.

(b) Periods given short with small Ratio.

Estimated Mean less than Arithmetic Mean. In *two* cases nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{4}$	474 } 1936 }	1205	958	1243	+ 38	+285	86	7
$\frac{1}{1.5}$	844 } 1318 }	1081	1067	962	-119	-105	77	8
$\frac{1}{1.1}$	1112 } 1277 }	1195	1192	1065	-130	-127	74	7

Number of Experiments, 78.

(c) Periods given long with large ratio.

Estimated Mean less than Arithmetic Mean, but in *no* case nearer Geometric than Arithmetic Mean.

R.	Periods	A. M.	G. M.	E. M.	Error		M. V.	M. V. %
					A. M.	G. M.		
$\frac{1}{8}$	1210 } 9682 }	5446	3423	5223	-223	+1800	194	3.7
$\frac{1}{7}$	1339 } 9579 }	5459	3581	5792	+333	+2211	381	6.5

Number of Experiments, 47.

§ 4. THE PSYCHOLOGICAL IMPORT OF THE EXPERIMENTS.

I. The fact brought out by the first set of experiments is that short durations ($\frac{1}{3}$ — $1\frac{1}{2}$ sec.,) are *overestimated* while longer durations (2—4 sec.,) are *underestimated*.

What psychological explanation can be offered for the *over* and *underestimation* of short and long periods respectively?

To seek any explanation demands a close analysis of the influences under which the subject makes his estimation. Many of the circumstances are what one may properly call accidental, variable from one experiment to another, one subject to

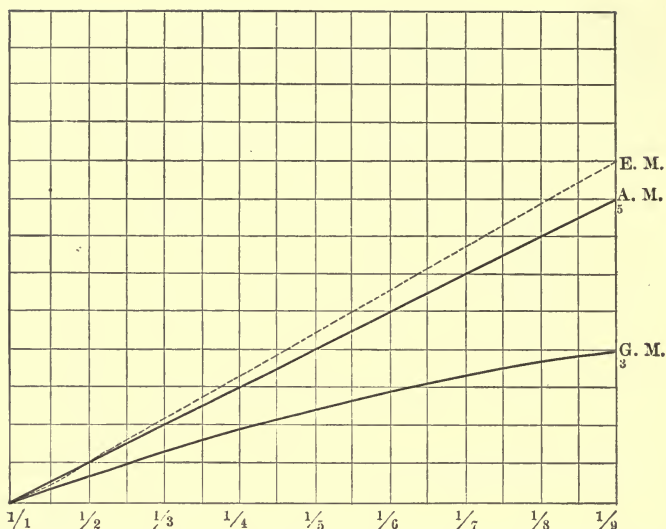


FIG. 7. Diagram showing the relation of estimated mean to A. M. and G. M. at various ratios. (Based on tables for Subject S.)

another, and without influence, therefore, on the average general result. Other circumstances are causal, but may, nevertheless, be peculiar to the method of experiment adopted and not essential to the estimation as such. It is the separation of these from the essential conditions which is difficult.

(a) The procedure was in a special sense procedure without knowledge. In his estimation and imitation of the duration the subject could rely only upon his actual experiences. There was no understanding between experimenter and subject that the period to be estimated should be kept the same throughout any series of experiments, though it usually was so, in order that any improvement in estimation due to Practice might be seen and taken into account. This entire absence of knowledge is not possible where a Gradation Method is used. There the subject knows that of the two durations given him, one is longer than the other. He knows further that there will be gradual change until equality is reached, and then further change until the original relations are reversed. The effect of such knowledge, like that of its absence, is seen, not in the general nature of the judgment, but in its degree of delicacy and in the con-

stancy of its pronouncement. With an Error Method, therefore, one must expect a larger Crude Error and a larger Mean Variation than in a Gradation Method.

(b) Error due to Position could not be influential, since only one period was delivered to the subject. By choosing the length of the period for each series of experiments with as much irregularity as possible, the error of Position was eliminated between series and series.

(c) So far the conditions of method considered have been negative. It was a positive condition peculiar to the method that the estimation took place between a memory image and an actuality. The subject imitated his memory image. The contrast between the idea and that which is actualizing itself in the present might be expected to have some influence upon the estimation; but the influence, whatever it may be, should be present in all estimations alike, and will not, therefore explain the *overestimation* of short intervals and the *underestimation* of long.

(d) It was also a positive condition that while making his estimation, while imitating his memory image, the subject was receiving muscular sensations from his operations on the key, which sensations might have influenced his estimation. This circumstance, however, would be present in both short periods and long, and if causal, would have a constant tendency in both alike.

The essential conditions of *over* and *underestimation* must vary with long and with short periods, since different effects mean different causes. One such set of conditions might be Dilatation or Contraction of the Memory Image. If it were a law that Memory Images dilated or contracted with lapse of time, so as to become of longer or shorter duration than their originals, the difference in time which must elapse between the beginning of the formation of the image and its imitation in the case of long and short intervals respectively, would influence an estimation based on Memory Images by rendering these Images of unequal growth or of unequal shrinkage.

Some separate experiments were made upon this point, but no law of dilatation or contraction could be deduced. Even if such conditions were at work, their effects in so brief an exist-

ence of image could not be sufficiently appreciable to explain the differences in estimation.

The differences between the two sets of periods, short and long, is so restricted that it seems right to reject as insufficient any explanation based solely upon an analysis of the sensations and after-images received. The question here is not primarily upon what data is Time Estimation based, but what are the conditions which make the estimation so different in the case of short and longer periods respectively. The ground of explanation seems to the writer to be in another direction. The usual predication of Duration as an attribute of sensation on all fours with Quality and Intensity, is apt to obscure the fact that, properly speaking, for Psychology Duration belongs only to the Dynamic view of Consciousness, *i. e.*, to Consciousness viewed as a stream, as a life of interconnected processes. To predicate Duration of an isolated sensation, is, from the *subjective* standpoint of Psychology, unthinkable. It may be said, 'This is a metaphysical difficulty,' but is not rather the contrary true? It is the metaphysical standpoint which makes us predicate Time of the element which we are avowedly considering in abstract. Duration must be the abject creature of the Law of Relativity whatever partial freedom Quality and Intensity may claim. It would seem, therefore, that it is in the Dynamic view of Consciousness that the explanation for the phenomenon of *over* and *underestimation* should be sought.

Rarely is the stream of our conscious life so narrow that we can detect in it only a single process or current. Our estimation of Duration is bound up with this plurality. We measure process against process; one endures, others come and go, this is long, those short. Should we be conscious of one current alone, *e. g.*, the interesting story our favourite novelist is unfolding to us, we lose count of time, and only realize it subsequently by noting other currents which have flown on unnoticed beside it and which we now recover, or by an appeal to some objective standard. In the experiments under consideration, every effort is made, as a preliminary step, to narrow the stream of consciousness. The subject is asked to concentrate his attention on the task before him, he waits especially

for the sound sensation. When it comes what happens? As far as the writer is able to analyze the situation, it seems that with a sound of short duration, the sound is the sole object of consciousness. Consciousness is through the concentration of attention for one brief interval dammed into a single current, it is all sound. For the after consciousness such an occurrence is a big event. There are not recoverable any temporarily unnoticed processes running along side of it, with which it can be compared. It stands in "splendid isolation" separating the before and after, as far as moments of a stream can be separated, and through its isolation it is *overestimated*.

With longer duration the situation is different. There is a limit to concentration. Consciousness is not all sound, other processes flow on beside the sound; muscular and organic sensations, fleeting images, passing thoughts, are present to consciousness. The process has relation to other processes, and the greatness which comes from mere absence of comparison is lost to it, thus there is no ground for *overestimation*. But through the presence of these other processes, it not merely loses its abnormal proportions, but suffers actual diminution. There has been diversion of attention and hence loss in mental value, so that the sound process seems to consciousness less than what its objective value warrants.

Against this attempt at analyzing the situation, the charge may be brought that it attempts to explain by having recourse to that which itself stands most in need of explanation—attention. That is true, but it is at least one step in the analysis if one can realize what function this *Deus ex Machina* performs.

II. The second set of experiments brings out the fact that Duration does not obey Weber's Law. The interest in ascertaining whether Duration does or does not lie under the great Sensation Law, lay partly in the indication this might give of the similarity or dissimilarity between Duration and Quality or Intensity, but chiefly in its connection with a theory of psychological measurement.

The question whether it is possible to have a system of psychological measurement must always have a methodological, if not a practical interest for psychologists. Ebbinghaus in his recently published "Grundzüge der Psychologie" sets forth

a system which is founded directly upon Weber's Law in such a way that the difficulties of Fechner's unit are avoided. The unit for measurement is "a directly experienced difference or distance between two sensations of the same kind, which are compared with one another in any respect." (*Grundzüge*, S. 63.) That we can perceive equality of differences is for Ebbinghaus a fact. "The equality of the steps here is no hypothesis or conventional assumption." (*Ibid.* S. 502.) Given a unit difference we can measure by means of it the difference (in either a positive or a negative direction) between any one sensation chosen as zero and any other.

The equal differences between sensations correspond to a Geometrical Progression between stimuli. Hence the formula. "The Intensity of sensations grows proportionally to the log. of the corresponding stimuli." (*Ibid.* S. 509.)

If the clearly perceptible differences which bear this relation to stimuli are known to be equal, then the least perceptible differences which bear the same relation must also be equal. That is, Fechner's hypothetical unit of measurement is to be accepted as valid practically, being deduced as a consequence of Ebbinghaus's empirical unit.

In this way Ebbinghaus saves the Fechner hypothesis from rejection, and yet at the same time does not use it as foundation for a system, a function which the results of recent experimental research seem to show it incapable of fulfilling. (Külpe, "Ueber das Verhältniss der ebenmerklichen zu den übermerklichen Unterschieden," Paris Congrès 1902. Ament, *Phil. Studien*, Bd. 16. S. 180.) The system, however, is founded upon what appears to the writer an unwarranted assumption, viz., that equality of sensation differences is something empirically verifiable.

Weber's Law gives us a certain functional relationship between stimuli and sensations—nothing more; the psychological unit needs independent proof. That the differences corresponding to a Geometrical Progression of stimuli are equal has to be demonstrated. Ebbinghaus cites the judgment of painters and embroiderers as to equal differences of shade. There is no doubt that such people term differences equal or multiples of each other, but what do they mean by it? It seems as though

ultimately they mean one of two things—either they mean that objectively, with reference to the amount or strength of pigment, there is equality or multiplicity, or, and this is the commoner case, they mean that they can imagine the same number (or a multiple of that number) of intermediate steps in passing from one shade to the other. If pressed as to their warrant in supposing these intermediate steps equal, they would finally take refuge in least perceptible differences, *i. e.*, we should be back at Fechner's hypothesis and must reverse Ebbinghaus's deduction of equality. Fechner himself seems to have recognized that it was the equality of least perceptible differences which was fundamental for the equality of the clearly perceptible. "Die Unterschiedsmassformel ist als das *Allgemeinere* der Unterschiedsformel, und hiemit auch der Massformel und Fundamentalformel anzusehen, sofern diese selbst besondere Fälle der Unterschiedsformel sind." (*Elemente der Psychophysik*, II, p. 103.)

If one considers what a difference between sensations in their Intensity or Quality is subjectively, one may the more easily realize how difficult it is to give a meaning to the "equality" of such differences, and echo the words of Kries, 'A measurement of intensive quantities will not be theoretically possible, until it is arbitrarily determined what is understood by "equality."' (Kries, *Vierteljahrschrift für wissenschaftliche Philosophie*, 1882. p. 352.)

We may allow with Ebbinghaus that the difference between quality *x* and quality *y*, or intensity *x* and intensity *y*, is sensational, *i. e.*, directly bound up with the sensations of the qualities or intensities *x* and *y*, but it will not itself be a sensation or a quality or intensity of sensation, and what is meant by its being 'equal to' or 'a multiple of' another difference between sensations, is hard to see.

It was this difficulty which suggested the experiments upon Duration. Suppose Duration to be an attribute of sensation. Then since differences between sensations in respect of this attribute are—both from the objective and subjective standpoint—Duration, equality of such differences would be intelligible. Here, at least, therefore, was a direction where Ebbinghaus's unit for measurement might be used.

If estimation of Duration showed agreement with Weber's Law, then a measurement system according to the well-known formula, $E = k \log. R.$ would be possible.

Since estimation of Duration does not follow Weber's Law, it seems to the writer that Ebbinghaus's system of measurement fails to have validity for any aspect of sensation.

CLASS EXPERIMENTS AND DEMONSTRATION APPARATUS.

By Professor E. B. TITCHENER, Cornell University.

I propose in this paper to make some suggestions regarding demonstration apparatus and lecture experiments suitable for a beginners' class in Psychology. Now that psychological instruction centres in the laboratory, rather than in the library, it is but natural that the old lecture courses should be replaced by courses in which demonstrations, class experiments and the projection lantern figure as largely as they do in elementary physics or elementary zoölogy. While, however, we are all, no doubt, using our laboratories to help out our class exercises, there has not appeared in print, so far as I know, any discussion of topics or materials of lecture demonstration. We find 'demonstration chronoscopes' and the like in the instrument makers' catalogues; we pick up this and that device from a colleague and work out this and the other for ourselves. We take what is on the market, and supplement it by things that appeal to our special interests,—a set of illusion charts, or of number-form models, or what not; we also exhibit pieces, taken from the laboratory, that are not demonstration pieces at all, but may be examined after lecture by those who are especially interested. Not very satisfactory! A good lecturer will, of course, always preserve his individuality; he will have demonstrations and methods of his own; he will be constantly improving upon ready-made apparatus. Still, there ought to be a full set of ready-made pieces, available to those who desire it at moderate cost. For, on the one hand, there are certain experiments that every student of psychology has a right to expect, and to share in, however poor the equipment of the departmental laboratory; and, on the other, we cannot begin to improve till we have a basis to improve from.

When, therefore, the Chicago Laboratory Supply Co. expressed a wish, some two years ago, to make up a regular set

of demonstration apparatus that might be supplied complete to new laboratories and departments, I gladly promised to give the matter my attention. I am not able, at the present time, to make any final recommendations. I have, however, got together a mass of notes upon one thing and another, and the issue of this Commemorative Number seems the right occasion to make a beginning with my proposals. Only a small portion of the ground will be covered here. I hope, before very long, to publish a more comprehensive survey of the field; and I hope, also, to have a set of apparatus on the market by the autumn of 1904.

In considering the subject of class experiments, we must be careful to distinguish between experiments that are performed, psychologically, by the student, and demonstrations that are made to the class by the lecturer. If I set up for fixation an illuminated circle of colored glass, and my audience observe the consequent after-image; or if I blow a double bicycle-whistle, and the audience observe the resulting difference-tone: then the class experiment is a true psychological experiment. Each member of the class does his introspection, sees and hears or fails to see and hear, for himself. If, on the other hand, I set up a reaction apparatus on the lecturing table, take my own reaction time, and write the sigma on the blackboard, I have made a mere demonstration. The class has seen the hand of the large clock start, and seen it stop; but no one, save myself, has any idea of the contents of the reaction consciousness. I have even known a lecturer, after showing some simple stereoscopic slides, to put the stereoscope to his own eyes and describe the perception,—all in the happy assurance that he was experimentally illustrating a fact of psychology. Now demonstrations of this second sort have their usefulness. It is much better to bring a stereoscope with you into the lecture-room, to pass round slides, and to work out the construction of the instrument by a diagram, than simply to talk about the facts and theories of binocular vision. And, again, there are many cases in which true introspective work on the part of the class is impossible. If, for instance, I am demonstrating the method of minimal changes by tuning-forks, or the method of right and wrong cases by the sound pendulum, or the method

of mean gradations by three color-mixers with black and white discs, I can give opportunity for a few crude introspections, but I cannot, obviously, carry out the method in detail: I cannot, under the conditions of time and place, go through a full series of observations, or lead my hearers to analyze their method of judgment and appreciate the dangers of secondary criteria. All this we may grant. Having granted it, however, we must insist that, within or without the lecture-room, the only psychological experiment is the experiment that requires the student to introspect. It may be worth while to demonstrate the sphygmomanometer, but the psychological lecturer can do no more with it than the physiological. We are teaching psychology; and, for that reason, our aim and ideal must always be to make our class demonstrations, so far as may be, true psychological experiments. The student who enters the laboratory from a general lecture course should not be wholly innocent of the introspective attitude. When we are showing how things are done in psychology, we should say so clearly, and explain why we can do no better; but, whenever possible, we should call on the class to do psychology for themselves.

The demonstration apparatus which I have in mind are, then, apparatus which shall subserve this latter purpose: apparatus that shall standardise the conditions for such introspections as the lecture-room and the lecture-hour allow. In what follows, I deal only with quality of sensation. In this field, the instruments fall roughly into two groups, according as the sense appealed to is capable or incapable of 'action at a distance.' For sight and hearing, single, large pieces may be placed upon or above the lecture table. For cutaneous sensation, a separate instrument must be provided for every student or pair of students.

I. VISUAL SENSATION.

We require, first, materials for the demonstration of the two great visual series, colors and brightnesses. This demonstration is most important: young students are, in my experience, astonishingly ignorant of grays and colors, and the statement that there are some six hundred distinguishable brightness qualities is, unless backed up by a demonstration, simple

Greek to them. The demonstration of grays is not difficult. We have the Marbe mixer, which can be made to give a slow-changing continuous series of brightnesses from Hering's cloth-black to his baryta white. We have, further, for a discrete series, the album of Marbe photographic grays which is listed by Zimmermann. Fortunately for laboratories with short purses, the latter demonstration is the better, as well as the cheaper of the two. As the album is opened out, the student is impressed by the large number of readily distinguishable qualities, and when it is fully open the extreme black and extreme white are present simultaneously in his field of vision. Of course, the number of grays shown is relatively very small. But the lecturer can go on to compare the photographic black with Hering's cloth-black: that, again, with the black of velvet; that again, perhaps, with the black of a lightless space (Kirschmann photometer tube, made of cardboard on a large scale); while the photographic white may be compared with Hering's baryta paper. But even taken by itself, the album makes a better demonstration than the mixer.

In the case of colors, we are not so fortunate. It is really surprising—and it is this sort of lack that justifies the present paper—that no dealer in artists' supplies has lithographed a 'psychological spectrum.' We have the Prang spectrum chart: useful enough, but psychologically incomplete, because it does not give the purples, and psychologically unfair, because of the uneven space distribution of the various qualities. We have also the Prang Standard of Color: useful, as giving an idea of the immense number of visual qualities, but again incomplete, and too small for lecture purposes.¹ A 'psychological spectrum' (the term is unhappy, but seems to have made its way into current terminology) would consist of four bands of color, the one running from red to yellow, the next from yellow to green, the third from green to blue, the fourth from blue through purple to red. For demonstration purposes, at any rate, there can be no objection to having these bands of equal length.

¹ I am not blaming the Prang materials for failing to serve a purpose that they were never intended to serve. They are, as I have said, very useful in a psychological laboratory. But they were not designed for psychological ends.

They should be printed upon stout cardboard, and so hinged together that the 'spectrum' can either be shown as a single line, or bent upon itself to form a complete square. The separate strips might well be 15 cm. in height and 30 cm. long. In the meantime, until something of the kind is manufactured, the lecturer will find it worth while to provide himself with a set of four hand-painted strips, over which slides a black cardboard cover, with adjustable vertical windows.

Our second problem is the separate demonstration of the three moments in a color sensation,—color-tone, saturation and brightness. We want to show that each one of these factors may vary while the other two remain constant. The apparatus that naturally suggests itself is the color mixer. Some form of color mixer is to be found in every laboratory, so that the advantages of the instrument need not be discussed here. I may, however, mention that I have now in course of construction a demonstration mixer, consisting essentially of a horizontal shaft, running parallel with the front edge of the lecture table and driven by a motor beneath the table, which by means of a set of friction plates may be made to turn, at any required rate of speed, one or more of six discs (large size Hering or Zimmermann) arranged along the table at 30 cm. intervals. I shall publish a full account of the apparatus later. It is obviously a great convenience to have six demonstrations ready, at the beginning of a lecture (the discs may be screened till they are required by the lecturer), and to be able, by a mere turn of a handle, to throw this or that or all the mixers into play. The apparatus has the further advantage that one's assistant can be adjusting a new set of discs to mixers 1 and 2 while one is showing discs 5 and 6, etc.: a good deal of time is lost, where two or three mixers only are used, in the unscrewing and resetting of the discs.¹ I may add a word of caution. The compound discs employed in demonstration must be put together before the lecture hour. Be sure, then, that they are tested by the assistant a short time before the lecture

¹Sometimes a demonstration must be given twice, at either end of the apparatus. The angle under which an equation is observed by the audience may make a great difference in the result of the demonstration.

begins! The color mixer is, in general, a safe demonstration: the equations that hold for your own normal eyes will hold well enough for your audience. But I have seen a lecturer entirely nonplussed by the fact that his equations, made up on the preceding afternoon, refused to equate during the morning lecture.¹

To return to the problem: I am accustomed to begin this set of demonstrations by keeping brightness constant while color-tone and saturation vary. To this end, I first mount a light color, say yellow (small disc), and a black and white (large discs), on a single mixer. On another mixer I mount a dark color, say green (large disc), and a black and white (small discs). I then show, by making my gray very much too light and very much too dark in both instances, that it is possible to estimate the relative brightness-values of a color and a gray, and therefore possible to equate these values. I next set the black and white of the first mixer to a brightness-match with the yellow: the match, which I have verified beforehand for my own eyes, will hold for at least a good part of the audience. Then, on a third mixer, I show the green *plus* white matched with the gray which has just matched the yellow. The equation, again, will hold for a good part of the audience. Finally, on a fourth mixer, I show the two matched colors, the yellow and the whitish-green. The brightness-values are the same; color-tone is different; the papers may be so chosen either that the saturation is also plainly different, or (if the lecturer prefer) that it is now sensibly the same. The contrast effects of the experiment are negligible. No more knowledge is required of the class than that black and white mix to make gray, and that the addition of black or white to a color darkens or lightens it.

To keep color-tone constant while brightness and saturation

¹I have tried to make out, empirically, by noting the results of laboratory practice, the range of variation of the colored-paper components of a color equation. The different papers of a set differ so much and in so many different ways that it is difficult to lay down even an approximate rule. It is, however, safe to say that, up to three component papers, the change of illumination (summer and winter, morning and afternoon) may condition a variation of 10%.

vary, it is only necessary to add in different amounts of black, white, or black and white to a disc of a given color. Two mixers are sufficient for this demonstration. The appreciation of differences of brightness, as distinct from differences of saturation, is rendered possible by the result of the foregoing demonstration.

To keep saturation constant, while color-tone and brightness vary, I usually take a standard yellow, and a hard, staring blue. The blue is very much the richer, more saturated color. I add to the blue more and more of a dark gray (little white with a good deal of black), until the class begins to recognize that the two saturations are approaching equality. I then set the blue-gray mixture at the point which I have previously determined as the point of equal saturation, and show it and the yellow side by side. Color-tone and brightness are obviously different; saturation will be, for the majority of the audience, sensibly the same. Two mixers suffice again for this demonstration.

Theoretically, it should be possible to reverse these demonstrations: to vary one of the three components while keeping the remaining two constant. Practically, the reversal has been made only in the case of saturation. By using a Kirschmann disc,¹ one can show all degrees of saturation of a color-tone, without changing the brightness-values. If the laboratory possess a set of Hering grays, it is not difficult to find a paper that shall match a color in brightness; the color leaf may then be pasted directly upon the gray ground, and the making of the disc is much simplified. It would, doubtless, be possible to construct, empirically, a disc that should show one tone and one degree of saturation, while the brightness-values of concentric rings increased or decreased from center to circumference, and to construct a disc of uniform brightness whose concentric rings should show different color-tones of the same saturation. So far as I know, however, these discs have never been made.¹

¹This *Journal*, VII, 1896, 386; IX, 1898, 346.

¹Since this paper was written, I have devoted some odds and ends of time to the theory and practical construction of the discs. I have also had some correspondence with Dr. Kirschmann about them; and

So much for preliminaries. We come, in the third place, to the demonstration of the laws of color mixture. I have treated this subject fully in my *Laboratory Manual*, and have nothing of importance to add in this place.¹ I will only mention that, in all my experience of colored papers, I have never been fortunate enough to find a pair of ready-made complementaries, except in the single case of blue and yellow. Hence I am compelled to demonstrate the first law of mixture directly by aid of these qualities alone.²

Fourthly, there are the demonstrations of local adaptation and after-images. For color after-images, I know of nothing better than the Wundt demonstration apparatus. For local

I hope that we may shortly be able to publish a joint note upon the subject.

¹Vol. I, S. M., 5 ff.; I. M., 9 ff. I have here omitted to say anything as to the best method of cutting paper discs for the mixer. We employ (and I believe that the manufacturers follow a similar method) two discs of copper, turned accurately to the right size, and pierced at the center. The paper is laid between the discs, and trimmed around the edge with scissors.

²I cannot be sure that we have tried, in the Cornell Laboratory, every available combination; but we have certainly tried a very large number,—and always, save in this one case, without success. If any of my colleagues can suggest other complementary pairs, I shall be grateful for the information.

I may say that the references to colored papers in the literature seem to me greatly to overestimate the permanence of the colors. Colored papers change their color—certain components fading out more quickly than others—even in the course of a few weeks. In some cases there must be an intrinsic chemical change in process; compound equation discs that I have laid away in the dark have proved, after a lapse of some months, to be worthless for their original purpose. Hering's cloth-black fades noticeably in a very few days: an annoying thing, when one is doing quantitative work!

If the lecturer has the time—and the materials—it is worth his while to supplement the mixer demonstrations by the mixture of solutions, as recommended by N. von Klobukow (*Vorlesungsversuch zur Demonstration der Wirkung von Complementärfarben und Farbungemischen beim Zusammenbringen von gelösten Farbstoffen*, Wiedemann's *Annalen*, xliii. [cclxxix.], 1891, 438). The procedure is admirably suited to bring out the difference between mixture of pigments and mixture of sensations—or psychophysical excitations. The reference, curiously enough, has escaped the notice of the *Zeitschrift bibliographers*.

adaptation and brightness after-images I employ the following arrangement. A large wooden frame, roughly 75 cm. across by 120 cm. high, is hung upon the blackboard above the lecture table. The frame is painted of the same dull black as the blackboard, and its lower half is faced with a thin board similarly blackened. At the back of the upper (open) half of the frame is fastened a sheet of dark gray cardboard (68.5 cm. across and 56 cm. high). The upper and lower halves are separated by a narrow horizontal shelf, which is concealed behind the upper edge of the blackened front, and which can be turned down by a catch at the side. Standing upon this shelf, and running in grooves cut in the vertical sides of the frame, is a framed cardboard,—the right hand half covered with Hering cloth-black, the left hand with brilliant white paper. At the center of the line of junction of black and white is placed a white-and-black ivory button, for fixation. The demonstration runs its course as follows. The black and white card is placed upon the shelf, and the class fixates the button for 30 sec. During this time the white and the black tend both alike towards a neutral gray; this change, and the intense black and white after-image borders due to slips of fixation, are noticed by all. At the end of the 30 sec., the catch is released; the shelf turns down; the black and white card drops behind the blackened front upon a layer of felt laid at the foot of the frame; and the class sees the complementary after-images form upon the dark gray background. The apparatus admits of demonstration to at least 250 students.

For contrast there is nothing better than the Hering window. It is quite easy so to regulate the lights that the contrast color shall be pronounced the richer, more positive, more saturated color of the two,—though, of course, a reference to the backgrounds will indicate at once (to one who thinks!) which is the 'objective' and which the 'subjective' color. Unfortunately, the window requires a dark room, and is not in any case adapted for demonstration to a large audience. My dark room adjoins the lecture room, and I pass my students through, in groups of 15, during the last quarter of the lecture hour, while other demonstrations are given at the lecture table by an assistant. The window affords, without any question, the most striking

demonstration in the whole field of visual sensation. For desk demonstrations, we may have recourse once more to the mixer. Helmholtz discs¹ may be prepared in great variety, and with judicious choice of colors and gray may be made to give brilliant contrast effects. The amount of contrast may be indicated by setting up, on a second mixer, a disc of the contrast color (colored paper with black and white) which has been matched to the induced color immediately before the lecture hour. This quantitative demonstration, however rough, is valuable: the number of degrees of objective color necessary to reproduce the contrast effect is oftentimes surprisingly large. I also give demonstrations of Meyer's Experiment. A sheet of colored cardboard is nailed to a stout wooden frame, and a sheet of tissue paper (weighted below with a strip of cardboard, so as to hang evenly) is gummed to its upper edge. The frame is hung upon the wall behind the lecture table. Strips of gray cardboard are slipped in between the tissue paper and the colored background. The tissue paper may be turned up (by the cardboard strip along its lower edge) without creasing. In this form, the apparatus is relatively permanent; the same colored backgrounds and tissue sheets may be used for several years. If several thicknesses of tissue paper are affixed to the same frame, it is possible to show that the contrast effect is not (as is still erroneously held in some quarters) a function of diminished saturation.²

¹ *Phys. Optik*, 1896, 544.

² Since writing the above, I have had the following apparatus constructed. Four sheets of Hering paper (red, yellow, green, blue) are pasted smoothly upon sheets of metal set flush with the front of a heavy black wooden frame. The papers are arranged from left to right in the order given; they are placed with the longer side vertical, and are about 7.5 cm. apart. Running horizontally across the centre of frame and colored spaces is a squared groove, in which can be laid a hard-wood bar, faced with a selected quality of gray paper. A lighter frame, with four tissue-paper windows is hinged to the upper edge of the main frame, and shuts down closely over gray bar and colored sheets. The bar can be shown upon the lecture table, and the uniformity of its gray facing seen by the audience. It is then placed in the groove, and the lighter frame locked down over all. Despite the differences of brightness and saturation of the four colors, the contrast effect, if the gray has been carefully chosen, is extremely well-marked.

Sixthly come the phenomena of indirect vision. These I demonstrate, very crudely, as follows. A strip of manila paper, 25 cm. high and about 2 m. long, pasted firmly to a wooden frame, is hung horizontally at a convenient height on the wall behind the lecture table. A black fixation-cross is painted at about 15 cm. from either end of the paper strip.¹ A light wooden rod, of the same color as the paper, carries discs of colored paper, 6 cm. in diameter. The class fixates the crosses alternately, with the appropriate eye, and the discs are moved slowly out and in along the manila strip. Rough as it is, the demonstration works very well,—and I do not know that anything else is required of a demonstration. In this, as in most other cases, I have upon the lecture table various laboratory appliances of the more refined sort: in the present instance, more particularly, an accurate perimeter, which I 'demonstrate' in a physical way. I am now concerned, however, with experiments which the students perform for themselves.

The effects of color blindness are shown, seventhly, by the aid of Holmgren worsteds. I keep two sets of these wools permanently arranged as they were sorted by two partially color blind observers, and hold up the matched skeins to the class. This is, in my experience, a better demonstration than can be made with colored charts. Actual tests for color blindness are taken, not with the worsteds, but with the Hering apparatus.

Lastly, the Purkinje phenomenon is demonstrated, as Sanford recommends, by requiring the class to observe a selected red and blue with nearly closed eyes.² The demonstration is more convincing if the papers are carried from daylight into a dark room, or if the lecture room is gradually darkened; but a partial closing of the eyes answers very well.

II. AUDITORY SENSATION.

We ought to begin, in the domain of hearing, with demonstrations of simple tone and simple noise. Unfortunately, the psychology of noise is still in a very unsettled state; we do not

¹ This apparatus may also be used for demonstration of the blind spot.

² *Course in Experimental Psychology*, 1898, 142.

know, as yet, whether there is a series of simple noises whose qualities differ from those of tone, or whether the 'pitch' of a noise is always identical with the pitch of a tone. Of the analysis of complex noises, except in temporal regard, we know practically nothing. All that one can do, then, as a first demonstration, is to set off a single noise against a single tone: I use a dull thud (such as is made by the drop of a leaden ball upon a leaden block) and the tone of a blown bottle. I then point out the tonal character of certain apparently simple noises: the snap of the finger against the ball of the thumb, the pop of a cork drawn from a phial, the stroke of a wooden hammer upon a block of wood,—employing in the two latter cases a series of bottles of different sizes, and the blocks of a xylophone. I then take the hiss (from a pipe or a Galton whistle) and the clatter (from a toothed wheel), and point out that the former is not, while the latter is, capable of introspective analysis. Next, I contrast the tone (tuning-fork, blown bottle) with the clang (reed, string), promising a future discussion of clang-tint. Finally, I show how a clang may be generated from noises (toothed wheel) and a noise from clangs (group of tonometer reeds or piano keyboard). This is all rather unsystematic; but we can hardly do more until knowledge has advanced beyond its present stage.

The second set of demonstrations deals with the range of tonal hearing. For the upper limit of tonality, one may use small forks, steel cylinders, or a Galton whistle. From the lecturer's point of view there is, I think, little to choose between these three sources, though I personally prefer the forks. The lower limit can hardly be demonstrated; the tones are too weak. I show every year the large Koenig fork, whose vibrations can usually be seen from all parts of the lecture room, the Appunn lamella, and the Appunn wire forks. These last make the best demonstration; the vibrations of the larger forks are clearly visible, and some of the higher tones can generally be heard in the front part of the room.¹ For discrimination of pitch I em-

¹ Whether these forks can still be obtained I do not know. Their stems are made of some alloy, which is very brittle: the forks do not give perceptible overtones, but are apt, as an offset, to break in two at the handle. I have had the set reproduced in steel wire. The new

ploy both a pair of Appunn forks with riders, and a tonometer with 4 vs. intervals. The latter makes a very satisfactory demonstration.

To demonstrate clang-tint, it is advisable that the lecturer have at his disposal a representative collection of musical instruments,—and be able to play them. Especial attention should be given to what Külpe terms, somewhat misleadingly, 'clangs of indefinite pitch' (cymbals, triangle, drum).¹ Whether clang analysis be undertaken (sonometer string, with or without resonators) depends on the size of the class. It may be demonstrated by help of the Ellis harmonical, or the Appunn C-box and overtone apparatus. I explain clang relationship by means of a large Mach model of the piano keyboard. Two strips of wood, of the same length as the keyboard and carrying black squares at the points indicating the first dozen overtones of the lower C, run in grooves above the keyboard. The strips can be set for any pair of primaries, and the degree of direct relationship is shown by the place of coincidence of overtones.

The best 'universal' apparatus for demonstrations in tonal psychology is, without any question, the new-pattern Stern variator, actuated by the Whipple double gasometer. The variator is made by F. Tiessen, of Breslau; a set of four bottles can be obtained for a reasonable price. The new pattern does away with the inconvenience of quicksilver, and the apparatus is much more compact and manageable than it was at first. I have had the Whipple tanks rebuilt, with metal frame and valves, and have mounted them upon a wheeled platform. The new model takes up less room than the old, is more sightly, and can easily be moved from room to room of the laboratory. I expect to publish later a cut and description of this whole arrangement. In the meanwhile, there are cheaper and more accessible demonstration pieces. For the continuous tone series, one may borrow a siren from the physical laboratory. For beats, one may use cheap forks, vibrating over

forks are much more durable than the old; but many of them gave a shrill overtone, which must be cut off by a cloth sleeve like that furnished with the Appunn lamella.

¹*Outlines*, 304.

wide-mouthed bottles. For difference tones, one may use pairs of Quincke tubes, or a double bicycle whistle, or the Kœnig wheel and tube apparatus. This latter I have had built in a much lighter and handier form than the original instrument, which is needlessly heavy and clumsy.¹ For fusion, one may use the Quincke tubes, or forks, or the keyboard of a piano or harmonium or harmonical, or the Appunn interval apparatus. But all these purposes, and several others, are subserved by the Stern variator. Since this is, at the same time, a research instrument, it is, literally, an apparatus that no well-equipped laboratory can afford to be without. It is at least as valuable, in psychological acoustics, as is the multiple color mixer in optics.²

III. CUTANEOUS SENSATION.

For the demonstration of pressure spots I employ a simple instrument devised by von Frey. It consists of a stout horse-hair, waxed into a short bit of narrow-bore glass tubing, the ends of which have been smoothed in the flame of a Bunsen burner. Very little time and no particular skill are required to make up a hundred of these instruments, and by help of them the whole class can readily verify the existence of pressure spots, say, upon the back of the hand, and can appreciate the character of the pure sensation of pressure.

Cold spots may be found by passing the blunt point of an ordinary lead pencil slowly across the back of the hand. As, however, one cannot be sure, in these days of fountain pens, that every student has a pencil, it is better to make express provision for the cold as for the warm spots. I use long wire nails (the sort that the carpenters call 'spikes'), rubbed to a rounded point. These are kept in ice water or warm water, as the case may be, and are handed round by an assistant. In

¹Any of the Cornell apparatus described in this paper may be obtained, at cost of time and materials, from the laboratory mechanician, Mr. F. A. Stevens,—though I cannot guarantee speedy delivery. A selected set of demonstration apparatus will, as I have said above, be presently put on the market by the Chicago Laboratory Supply and Scale Co.

²The demonstration of the proper tone of the auditory passages, easily made by a piston whistle, is always interesting to a class.

the great majority of cases they hold their temperature long enough for demonstration purposes. Cold spots are easily discovered. I am not very sure about the warm spots. The best region to explore is the surface of the upper eyelid. But introspection of warmth sensations is difficult: the spots are less strictly circumscribed than the cold and pressure spots, and the sensation of warmth rises less quickly to its full intensity than do the other sensations. No doubt, the more accurate observers definitely obtain or as definitely fail to obtain pure warmth sensations. But I have thought at times that some of my hearers found their warmth spots out of complaisance to the lecturer. At any rate, the demonstration is one to be conducted with caution.

I make no attempt to demonstrate the existence of pain spots during the lecture hour. Nor do I appeal to anything but simple introspection for the analysis of the cutaneous-kinæsthetic complexes. Lecture table demonstrations may be made; but I know no practicable way of putting the class to work along with the lecturer.

IV. TASTE AND SMELL.

Demonstrations in these fields are very difficult. It is worth while, perhaps, to provide the class with small mirrors, for the identification of the taste papillæ, though this is hardly a psychological demonstration. It may be worth while to call some member of the class to the lecture table, to blindfold him and stop his nostrils, and then to show the confusion of various 'tastes:' but I have known this demonstration to result rather in embarrassment on the part of the observer and hilarity on the part of the class than in any definite contribution to the psychology of taste on either side. Indeed, whether one can do anything by way of demonstration with tastes depends almost entirely upon the size of the class.

The same thing is true of smell. It is not difficult to make up half a dozen sets of phials, containing scents from Zwaardemaker's nine classes;¹ but to make up and to keep in order fifty such sets require more trouble than the lecturer will be willing to expend upon the demonstration. The olfactometer

¹ See my *Laboratory Manual*, i, I. M., 127 f.

is best kept upon the lecture table. I once saw a lecturer and a roomful of three hundred students all violently holding their noses, in the effort to produce a smell sensation by mechanical stimulation. The result did not warrant the expenditure of energy.

Aronsohn's method of determining the olfactory qualities may be demonstrated by sets of three phials: exhaustion by camphor kills the scent of absolute alcohol, while it leaves that of musk practically unimpaired. But, apart again from the trouble of making up and renewing these sets, it must be remembered that scents readily diffuse in the lecture room, and cling for some time to the clothes of the students. Demonstrations of this sort are therefore liable to bring psychology into disrepute with other departments of the university.

V. OTHER SENSATIONS.

For all the remaining sensations I employ apparatus only on the lecture table. Sensations of tendinous strain may be got from the clenched fist; sensations of articular pressure from the moved wrist; the muscle sensation proper must wait over for laboratory experiments. The sensation of dizziness may be obtained, briefly but clearly, from a quick jerk of the head to one side,—as if one were trying to throw one's head away. The tickling complex may be brought to mind, if not exactly reproduced, by light movement of a finger-tip over the palm of the other hand. A concomitant sensation of shiver may often be set up by the squeaking of the writing-chalk upon the black-board. Hunger, thirst, nausea, muscular pain, lightness and oppression of breathing, tingling, itching, etc., must be left to memory or to chance experience.

These last paragraphs have a rather trivial appearance. I have, however, written out my own procedure, partly in the hope that some of my colleagues may be able to suggest better demonstrations from their teaching experience. In conclusion, I may say a word about models. I had meant in this paper to discuss the psychological use of the projection lantern; but limits of space forbid.

For vision, I use the large Auzoux eye model. For audition, I have the Auzoux ear model, Steger's large plaster

models of the internal ear (admirable demonstration pieces), and Helmholtz' mechanical model of drum-skin and ossicles. It is also worth while to make a rough model of the unrolled cochlea: the simplest materials are a wide tin tube, say 60 cm. long by 15 cm. in diameter, closed at the one end by a rounded cap (removable); a strip of stout white cardboard, of the same length and width as the tube, painted black over one half from the diagonal to the boundaries; and a narrower strip, with a bent-up foot, to represent Reissner's membrane. The model may be further elaborated, but is best made large and very simple.

I know of no psychologically adequate model of the sense-organs of the skin. All that one can do is to take some one of the ordinary physiological models, and paint or mould upon it the organs that one wishes to show. The mode of stimulation of the pressure and pain organs may be demonstrated by help of a thin board laid over a thickish block of sponge rubber.

In discussing the static sense, I have sometimes used, besides the Steger models, a large glass tube, expanded to a bulb near one end, and partially filled with a colored liquid. The remaining sense-organs do not require models, though certain of them (*e. g.*, muscle and tendon, the articular organs) require diagrams of a different sort from those found in the physiologies. I give in my *Primer of Psychology* (p. 48) a diagram which shows the mutual independence of the sensations of strain and of muscular contraction. Synaesthesia may be illustrated by painted diagrams, of the kind given by Galton in his *Inquiries into Human Faculty*.

EXPERIMENTAL STUDIES IN THE PSYCHOLOGY OF MUSIC.

By Professor MAX MEYER, University of Missouri.

- I. The Æsthetic Effects of Final Tones.
- II. The Intonation of Musical Intervals.
- III. Quartertone-Music.

I. THE ÆSTHETIC EFFECTS OF FINAL TONES.

There are two most important conditions under which the ending of a melody, *i. e.*, of a succession of related tones, has a particular æsthetic effect, an effect of satisfaction, of rest. One of these conditions is the falling inflection (which is effective also in unrelated tones), the other condition is the passing from a tone not represented by a pure power of 2 to a related tone represented by a pure power of 2, when the latter tone has previously been heard or at least imagined. I have elsewhere called this latter effect the tonic effect. The former may be called the effect of the falling inflection.

Some of the peculiar psychological consequences of the effect of the falling inflection are well known to elocutionists. Unfortunately, the matter has never been studied from a purely psychological point of view, important as it is for the psychology of speech as well as of music. I can therefore only briefly point out the difference in psychological effect of the rising and falling inflection in speech. The whole effect may, perhaps, be described as an effect upon the attention of the listener. (I mean here by attention mental activity in general.) A rise in pitch causes the hearer's attention to become strained, and the more so, the steeper the ascent, if I may use this expression. A fall in pitch, on the other hand, causes a relaxation of attention, a cessation of mental activity. No one while asking for any information uses the falling inflection. If he does so, indeed, he may be sure that he will never receive any answer. No one who desires to convince others of some truth with ultimate

success, excluding any further doubt, will use the rising inflection. If he does, he will at once see his hearers shake their heads and show their skeptical attitude. They continue to be mentally active, to keep the matter under consideration.

The same strain and relaxation of attention is to be found in music. The normal end of a mental process is, of course, characterized not by strained, but by relaxed attention; for strained attention means continued mental activity. It is natural, therefore, that a melody ends with a falling inflection. It is not, however, absolutely necessary that a melody end with a falling inflection. The composer may desire to produce the psychological effect of the rising inflection at the end of the melody, and he has the right to produce it.

The other peculiarly satisfactory ending of a melody is the passing from a tone which is not represented by a pure power of 2 to a related tone represented by a pure power of 2, *i. e.*, to a 'tonic.' I have elsewhere¹ applied this tonic effect more in detail to the theory of music.

The most common theory of the satisfactory ending of a melody asserts that a melody, to end satisfactorily, must move from overtones to their fundamental tone. The term 'fundamental' is significant enough in this connection. There is, of course, some truth in this assertion; otherwise it would never have been accepted. But this truth is very imperfect, for there are innumerable cases which it does not explain, and it can be reduced without remainder to the two laws above stated. 1. The fundamental tone is *lower* than its overtones; this is, therefore, a special case of the effect of the falling inflection. 2. The relation of the fundamental to its overtones is a special case of the relation of a tone 2 (using my own symbolization) to a related tone which is not 2, a special case of the tonic effect. One explains a fact scientifically by showing it to be a special case of one or more universal laws. This is what I have done above with the musical effect of the fundamental tone. What is generally found in textbooks is the attempt to explain all musical facts by reference to the special

¹ Contributions to a Psychological Theory of Music, University of Missouri Studies, I, 1, pp. 80.

case of the relation of a fundamental tone to its overtones. Of course, this attempt could not be successful.

The relation of a fundamental tone to its overtones is physically and mathematically so interesting that it is not wonderful that those who have studied the psychological facts but very superficially should have accepted this relation as a satisfactory explanation of all musical facts. I mean in particular the authors of textbooks on physics, some of whom not only present to their readers this superficial æsthetic theory based on fundamental and overtones as an established truth, but even go so far as to ridicule the psychologist who expresses his belief that a few problems in the psychology of music are yet left unsolved.

I will now report the results of a few experiments which clearly show the effect of the falling inflection. The subjects were a number of my students, of both sexes; some more, some less musical (the majority less). Three tones of a reed organ (dulciana stop) were played to them for a few minutes in irregular succession, in order to make the subjects familiar with these tones. After this preparation the actual experiment was begun. The three tones were played a few times in irregular succession, ending on one of them. Then they were played in a similar way, ending on another one; and lastly, ending on the third tone. This was repeated until each subject had made up his mind and written down which of these three endings was the most satisfactory to him. Though the whole number of judgments is but small, the result is characteristic enough.

Two classes of experiments must be distinguished: One in which there was no tonic effect among the three tones; and one in which there were tonic effects. In the former case the three tones were represented by the symbols 3, 5, and 7; in the latter, by 2, 3, and 9. The absolute pitch was always within the range of the human voice. As to the relative pitch, the tones were selected as close together in pitch as was possible in each case; *i. e.*, the three tones of one experiment were always within a single octave. Each of the three tones, however, had an equal chance of exerting its influence, *i. e.*, of being the lowest of the three.

H	7	2	H	5	1	H	3	2
M	3	2	M	7	2	M	5	4
L	5	6	L	3	7	L	7	4

The above little tables show how many times each ending was preferred to the other two possible. L means the lowest tone, M the middle, H the highest. The figures in the second column of each little table are the symbols of relationship. The numbers of the last columns indicate the number of preferences. Since there is no tonic effect in these cases, we may add the preferences for L, M, and H, each without respect to the musical symbols, *i. e.*, without respect to the relationship. We then have the following table:

(I)	High	:	5 times, 17 per cent.
	Middle	:	8 times, 26 " "
	Low	:	17 times, 57 " "

We see a decided preference (57%) for the lowest tone, a dislike for an ending on the highest tone (17%). With this result we must now compare the preferences when the tonic effect is included as a determining factor, as shown in the following tables:

(II)	High	3	:	0 times, 0 per cent.
	Middle	9	:	4 times, 14 " "
	Low	2	:	24 times, 86 " "
(III)	High	9	:	1 time, 4 per cent.
	Middle	2	:	19 times, 70 " "
	Low	3	:	7 times, 26 " "
(IV)	High	2	:	2 times, 7 per cent.
	Middle	3	:	15 times, 54 " "
	Low	9	:	11 times, 39 " "

In table II the lowest tone is the tonic. The combined forces of the tonic and the falling inflection have concentrated 86% of the preferences upon the lowest tone. In table III, where the tonic is the middle tone, it has attracted only 70% of the judgments. In IV, where the tonic is the highest tone, it seems to have lost its peculiar power altogether. However, to explain the distribution of the judgments in IV, we must make use of another psychological effect, which I have also previously emphasized in other publications. In II and III

the three tones are close together; they are all within the limits of a Fifth. As I have elsewhere pointed out, proximity in pitch makes related tones act as a unit, as parts of a psychological whole. The farther apart two related tones are, the less they act as parts of a whole, and the more as separate units; their mutual relationship is less effective. The melody is actually broken up into partial melodies. This is exactly the effect of combining 2, 3, and 9 in such a manner as in IV—9 and 2 are pretty far apart. In II and III the tone 2 was the tonic of the whole, of 9 as well as of 3. But now, because of the distance between 2 and 9, this tonic effect (9-2) has become considerably weaker. The melody therefore falls into two partial melodies, one represented by 9 and 3, the other by 3 and 2. The problem now is not simply which of the three tones shall be at the end, but rather, which of the two partial melodies shall be at the end. This is determined by the falling inflection. The lower partial melody is preferred at the end. And within this partial melody (9-3) its tonic makes its power effective. The tone 3, therefore, receives 54% of the judgments. Hardly any subject seems to care much for the higher partial melody; its tonic receives only 7% of the judgments. But this breaking up of the melodic effect gives the falling inflection an opportunity. The consequence is that the lowest of the three tones attracts a considerable number of the preferences (39%).

The falling inflection has never received from the psychologists the attention which it deserves. Its effects have been noticed, but have not been interpreted as special cases of a general law. It is impossible at present even to attempt to formulate this law. But some one will doubtless some time succeed in doing it, provided that psychologists become aware of the fact that there must be such a law, that these observations in special cases are not unrelated facts, but instances of the effect of a law of some significance. Special observations of this sort are numerous. I shall quote a few from a recent article by Whipple (*this Journal*, XIII (2), April 1902.) p. 231: "B prefers to have V move down; it is easier to react then than when V moves up. In the latter case, there is more strain and nervousness, greater expectation, and a change from the usual method of judgment, much attention being given to the

image." Another special case of this nervousness, set up by the rising inflection, is the muscular effect mentioned in the following, p. 264: "Feelings of tightening and relaxation for 'higher' and 'lower' respectively, were reported throughout the tests with discrete tones, and were also well brought out with the wide differences used in the reaction method."

Now, if a rising auditory sensation causes a peculiar state of attention, of nervousness, should not the reverse effect be possible? Should not a given condition of nervousness tend to raise the pitch of an auditory sensation; of course not of a peripherally aroused sensation, but of a memory image? That such is indeed the case will be seen from the following quotations, p. 240: "Taken with the prevalence of comments by the observers upon the ease with which the position of V is recognized when it does start from below, we find confirmation of the principle upon which we insisted in part I, viz., that for most observers, there is a tendency to sharp the image in the case of a long time interval. . . . The pitch of the auditory image has been gradually raised in the endeavor to maintain it as vividly and clearly as possible." P. 260: "The image apparently tends to flat, but this tendency is more or less consciously resisted by most observers, so that, at least at 30 seconds or afterwards, it is more often sharp." I suspect that the tendency of the image to flat, which Whipple mentions in the latter quotation, is caused by a mental condition opposite to that of nervousness, of continued activity, *i. e.*, by a condition of contentment, of tranquilization, by the dying out of some distinct mental process. Unfortunately, the introspections reported do not mention anything which might clear up this point.

II. THE INTONATION OF MUSICAL INTERVALS.

In an article by C. Stumpf and the present writer¹ may be found quite a number of facts concerning the intonation of musical intervals. There are, however, some problems left, and some others are suggested by the results of our experi-

¹Massbestimmungen über die Reinheit consonanter Intervalle. Zeitschrift f. Psychol. u. Physiol. d. Sinnesorg., XVIII, 1898, pp. 321-404.

ments made in Berlin. On two of these problems I have made further experiments, the results of which are here reported.

1. In an article on the theory of melody¹ I have reported some determinations of the actual intervals which seem to me to have been intended by the composer of a certain melody which we possess merely in the common, unscientific, musical notation. Lipps² has raised the objection that I might have been influenced by the experimentally proved tendency to deviate somewhat from the theoretically perfect intonation. I have never regarded this as a very probable source of error, since the deviations to which Lipps refers are not so great as the pitch differences in question. But, in order to decide this question finally, I have undertaken the following experimental investigation.

In my investigations concerning the intonation of certain melodies I always had to choose between two intonations, representing two theoretically different musical intervals. The difference between two such intervals was usually large, 5 or 10 or even more vibrations. From our experiments made in Berlin I knew that the average normal deviation from a perfect interval was never as great as this. However, the question then was a different one. A single interval was presented and the hearer asked if he thought it too large or too small or satisfactory. Now, let us regard a Major Third one vibration too large as subjectively most satisfactory; it is then *apriori* possible that an enlargement of this satisfactory interval by a certain amount would be less objectionable than a diminution by the same or even a smaller amount; *e. g.*, a Major Third four vibrations too large might be preferred to an interval one vibration too small. If this were true, my method of determining the intonation of melodies would be impracticable. But the result of the experiments made to decide this question was negative.

The subjects were partly professional musicians or amateurs of extraordinarily high musical training and ability, so that

¹ Max Meyer: Elements of a Psychological Theory of Melody. *Psychological Review*, VII (3), 1900, pp. 241-273.

² Lipps: Zur Theorie der Melodie. *Zeitschrift f. Psychol. u. Physiol. d. Sinnesorg.*, XXVII, 1902, pp. 225-263.

they must be classed together with the musicians; partly a number of my students, among whom I selected those who possessed the greatest musical ability. The former class comprised eight individuals who took part regularly in the experiments, the latter about a dozen. In the tables below I give first the results for the former class separately, and then the results for all subjects together.

The intervals used were the Octave and the Fifth. We had found in Berlin that in these two cases the deviations from the objectively perfect interval were particularly great. For this reason I selected these intervals. The tones were a C of about 270 vibrations, its higher Fifth and higher Octave, produced by reeds. The Fifth and Octave were represented by a great many reeds, differently tuned. The intervals under observation were always rising. The experiment was performed in this way: C was sounded, then G; then C again and then another G somewhat higher than the first. The whole experiment was then repeated a second time, and the observers had to write down, which of the two G's they preferred, the lower or the higher one. The given tones appear therefore in the tables always in pairs. The numbers mean the percentage of preferences. The percentage is calculated from a total number of 130 to 160 judgments in each case.

Fifth.

Variations of h. t.	-2	+1	-1	+2	0	+3	+1	+4	+2	+5
Musicians	5	95	29	71	67	33	77	23	95	5
Average mus. indiv's	13	87	40	60	66	34	81	19	94	6

Octave.

Variations of h. t.	-3	+2	-2	+3	-1	+4	0	+5	+1	+6
Musicians	0	100	9	91	55	45	79	21	88	12
Average mus. indiv's	4	96	13	87	43	57	75	25	85	15

The tables show, in perfect harmony with the results of the former (Berlin) experiments, that the subjectively pure Fifth and Octave (rising) are greater than the objectively perfect intervals; the Octave still more than the Fifth. But the answer

to the *new* problem is entirely negative. That is, if we have to choose between two given tones, we select the one which is closer to the subjectively pure interval, no matter in which direction it deviates from this interval.

The objection that experiments on the æsthetically most effective intonation of a melody might be disturbed by a tendency to deviate considerably in one direction rather than a little in the other, must therefore be pronounced unfounded, if the melody consists of two tones only. Now, should we have to assume that considerable deviations in a certain direction are required in a more complex melody, *i. e.*, in a whole system of intervals? Lipps asserts this. A melody, according to Lipps, represents emotions. And deviations from the theoretically perfect intonation are caused, according to him, by a tendency on the part of the hearer to render these emotions as characteristic as possible. If such is the case in a single interval of two tones, how much more in a system of intervals, a melody made up of many tones. Here these deviations, characteristic of the emotions represented by the melody, must be, according to Lipps, extraordinarily great, being the sum of the deviations characteristic of each interval.

I am unable to see either the logical strength of the conclusion, or any empirical foundation for this whole theory. In a single interval a deviation from the theoretically perfect intonation does not cause any disturbance of the conditions of perception³ beyond the perception of this very interval. But in a melody, the consequence of any deviation is an alteration of every other interval made up by a tone related to the tone which is altered in the first place. This, I should think, is so serious a consequence, that the assumption is more probable, that the tendency to deviate from the theoretically perfect intonation is much weaker in a melody than in a single interval. So far as incidental observation on my part goes, the deviations in a melody are indeed smaller than in a single interval. But I do not wish to assert this positively and make it the starting point for speculation, while exact experimental measurements⁷ are yet unavailable.

On the other⁸ hand, as to experimental facts, on which this theory of the deviations' being caused by the representation of

characteristic emotions should be based, I do not know any such facts. The facts reported in the following section of this chapter will, on the contrary, perhaps help to convince the reader that the causes of the deviations must be looked for elsewhere than in the supposed representation of characteristic emotions. This theory is merely an hypothesis, in which I do not believe, since I do not see its scientific usefulness.

2. The result of our experiments made in Berlin was that the Major Third, Fifth, and Octave are preferred a little larger than the theoretical intervals; the Minor Third, on the contrary, a little smaller.

Stumpf asserts that under certain conditions a Minor Third a little larger than the theoretical interval is preferred. But I do not agree with him in deriving this conclusion. The table on page 340 of our paper is not a '*Rohtabelle*,' but a very arbitrarily constructed table. The method employed there did not yield any regularity in the results, and in the table, therefore, the reeds producing the variable tone were arbitrarily combined into certain groups. But I am convinced that these groups were too arbitrarily formed to permit any conclusion. This table cannot prove anything. I feel quite sure that the Minor Third is generally preferred somewhat diminished.

The problem now before us is this: *Why* is the Minor Third preferred *diminished*, but the Major Third, Fifth, and Octave *enlarged*? Stumpf has given in our paper, p. 342, an explanation of this fact which I shall prove to be wrong. He has a theory similar to that of Lipps; *i. e.*, the deviations are caused by certain emotions or feelings. The difference between the Major and the Minor Third is thus explained by Stumpf: The Major Third causes a feeling of sharpness, the Minor Third a feeling of bluntness. These two intervals are very common in music. Therefore, when we hear one of them, we cannot help thinking of the other. Now, in general, according to Stumpf's theory, we like all intervals a little enlarged. But in the case of the Thirds, in order to make the characteristic feeling as strong as possible, and prevent its being weakened by the memory image of the other interval, we take the Major Third as large, and the Minor Third as small as possible.

My aim was to find out experimentally whether this expla-

nation of the intonation of the Minor Third was right or wrong. If the intonation of the Minor Third is influenced by the image of the Major Third, by 'contrast,' as Stumpf says, we should notice some difference in intonation proportional to the readiness with which the Major Third is imagined. I therefore made three series of experiments with the same subjects as in the previous section of this paper. In one series I added to the Minor Third the Fifth of the lower tone; in the second series I added the Minor Sixth of the lower tone; in the third series the Octave. The highest tone was sounded first; then the lowest and last the variable tone representing the Minor Third. The subjects knew that only the third tone was variable. The procedure was exactly the same as described in the experiments on the Fifth and Octave.

Minor Third plus Fifth.

Variations of Third	-3	0	-2	0	-1	+1	-1	+2
Musicians	15	85	33	67	64	36	96	4
Average musical individuals	28	72	47	53	61	39	90	10

Minor Third plus Minor Sixth.

Variations of Third	-3	0	-2	0	-1	+1	-1	+2
Musicians	13	87	20	80	56	44	98	2
Average musical individuals	22	78	35	65	53	47	91	9

Minor Third plus Octave.

Variations of third	-3	0	-2	0	-1	+1	-1	+2
Musicians	26	74	30	70	73	27	96	4
Average musical individuals	24	76	22	78	60	40	88	12

No one who has any musical experience will deny that our readiness to think of either of the Thirds is very great when we begin each experiment by sounding the Fifth or the Octave. But if we begin with the Minor Sixth and then hear the lower tone, there is a strong expectation of the Minor Third; but no expectation at all of the Major Third unless we

have just returned from Bayreuth. We should find, therefore, in accordance with Stumpf's theory of contrast, that a diminished Minor Third is preferred in combination with the Fifth and Octave, an enlarged one in combination with the Minor Sixth. The tables do not show anything of the kind. The only difference found is this, that the preference of a diminished interval is slightly less in combination with the Sixth than with the Fifth and the Octave. But this difference is so small that it is safe to attribute it to chance. We then reach this conclusion: The 'contrast' of which Stumpf speaks may have, under certain circumstances, a slight influence upon the intonation of the Minor Third; but *contrast is entirely inadequate for explaining* the fact that a diminished Minor Third is generally preferred. And then, we may go a step farther and conclude that the whole theory (of Lipps as well as of Stumpf) explaining the facts of intonation by characteristic feelings is mere fancy.

It is often interesting to note the extent to which a speculative theory is influenced by the usage of language. It is merely an *historical* fact that the intervals 5:6 and 4:5 are both called Thirds, the one Minor, the other Major. If we count the semitones, it is perfectly justifiable to call the interval 5:6 a 'Third' and the interval 4:5 a 'Fourth.' If, historically, the latter nomenclature had been accepted, no one, most probably, would ever have thought of explaining differences of intonation by contrast. But since these intervals are accidentally called Minor and Major Thirds, there must be, of course, 'contrast' between them!

After we have broken down let us reconstruct. The intonation of the Minor Third and the intonation of the other three intervals seemingly do not obey the same law! But why not apply a little mathematical thought? We found in Berlin that the deviation of the Octave is very great, of the Fifth less, of the Major Third the least. Is it not perfectly natural, under these conditions, to assume that the curve representing these facts passes through *zero* below the Major Third? Then, of course, the deviation of the Minor Third must be of a negative sign, as it actually is. Now, this is not offered as a mere hypothesis, but I shall prove that it is true. If the sign of the

deviation of the Minor Third is negative, we shall expect it to be negative also in the case of any yet smaller musical interval, and the absolute amount to be correspondingly greater.

I selected for experimentation the smallest interval of two related tones, the Semitone (15:16). The subjects were three, Prof. A., B., and the writer. A. and B. are amateurs of exceptionally high musical abilities. A. plays the piano and the organ and is also theoretically trained; B. plays the violin and has a memory for absolute pitch. The tones were produced by reeds of 403 and 448 vibrations and a number of reeds between. 403 was the starting tone for the rising interval, 448 for the falling interval. The interval 15:16 is represented by 430 in rising, by 420 in falling. For the judgment, each interval was given twice, with a short pause between. The writer (when serving as subject) knew of course the intervals used, but not the order in which they were offered for judgment. The other two subjects knew nothing and suspected nothing about the objective conditions of the experiments.

Semitone, rising.

Variation of the higher tone	Actual number of Judgments.			Judgments per cent.		
	—	o	+	—	o	+
— 5	0	0	9	0	0	100
— 6	0	5	16	0	24	76
— 7	7	9	5	33	43	24
— 8	10	20	3	30	61	9
— 9	22	11	0	67	33	0
—10	31	1	1	94	3	3
—11	20	1	0	95	5	0
—12	12	0	0	100	0	0

In spite of the fact that the writer knew that all the intervals were objectively too small, no difference was found between his judgments and those of the other two subjects. The judgments of all three are therefore added together. The minus-sign under the head of 'judgments' in the table means that the interval was declared too small, the plus-sign that it seemed to be too large. o means that the interval was satisfactory. It is astonishing to see how much a rising semitone must be diminished in order to be satisfactory. In the case of -5 the subjects repeatedly stated their dissatisfaction in very strong

terms, saying that it sounded almost like a whole tone. This fact shows how unsuspecting the other two subjects were, particularly in connection with the fact that each series with rising intervals was made in the same hour and following after a series with falling intervals, among which the perfect interval 15:16 frequently occurred and therefore was actually heard. That these subjects were perfectly normal, with respect to the Major Third, Fifth, and Octave, had been found in the other series of experiments. It can also be seen from the following tables showing the judgments of the same three subjects on the intonation of the Fourth and the Major Sixth. They preferred all intervals above the Major Third enlarged, and the more so, the greater the distance represented by the interval. No experiments were made by us in Berlin on the Fourth and the Major Sixth. That these intervals obey the same general law is proved by the tables.

Fourth, rising.

Variation of the higher tone.	Actual number of Judgments.			Judgments per cent.		
	—	o	+	—	o	+
—1.0	11	1	0	92	8	0
—0.5	15	3	0	83	17	0
o	18	3	0	86	14	0
+0.5	11	7	3	52	33	15
+1.0	5	11	5	24	52	24
+1.5	3	4	14	14	19	67
+2.0	2	4	15	10	19	71
+3.0	0	1	14	0	7	93

Major Sixth, rising.

Variation of the higher tone.	Actual number of Judgments.			Judgments per cent.		
	—	o	+	—	o	+
—1.0	12	0	0	100	0	0
—0.5	17	1	0	94	6	0
o	19	2	0	90	10	0
+0.5	19	2	0	90	10	0
+1.0	11	10	0	52	48	0
+1.5	3	13	5	14	62	24
+2.0	0	16	5	0	76	24
+3.0	4	2	9	27	13	60

We may therefore state it as an established law that the

smaller musical intervals are preferred diminished, and the more so the smaller the distance represented by the interval; that the larger musical intervals are preferred enlarged, and the more so the greater the distance represented by the interval; and that the point where the curve of the deviations passes through zero, is situated between the Minor and Major Thirds. But we must add a condition to the formulation of this law: namely, the interval must be a *rising* interval.

This condition could have been derived with some probability from the experiments made in Berlin. It is clearly proved by the following table.

Semitone, falling.

Variation of the lower tone.	Actual number of Judgments.			Judgments per cent.		
	—	o	+	—	o	+
—2	1	1	34	3	3	94
—1	2	1	33	5	3	92
o	3	19	14	8	53	39
+1	11	22	3	31	61	8
+2	24	11	1	67	30	3

The preference of a diminished interval is extremely slight in comparison with the same rising interval. The falling semitone-interval, in which the lower tone was sharpened by one vibration, rendering the interval one vibration too small, was declared in 61 % of the cases satisfactory. But this deviation from the theoretically perfect intonation is insignificant, when compared with the deviation of eight vibrations in rising. We must, therefore, add to the above formulation of the law the condition that this law holds good only for rising intervals. A possible explanation of the small deviations in falling intervals may be found in the fact that sometimes, in order to judge, we call up a memory image of the first tone and thus change the falling interval into a rising one.

I have thought of the question whether the effect of the rising and falling inflection which we have discussed above might have any causal relation to the difference of deviation in the rising and falling intervals. I am unable, however, to establish any causal connection between these facts.

III. QUARTERTONE-MUSIC.

By 'quartertone-music' I do not mean music which strictly speaking is made up of quartertones; but any music which contains intervals considerably smaller than a semitone. Such small intervals are usually called quartertones. Music of this sort is very common among Asiatic peoples. Numerous writers have pointed out this fact, but no attempt has ever seriously been made at a psychological theory of this music. One certainly cannot designate as an attempt at a theory the simple assertion that quartertone-music is based on psychological laws fundamentally different from those forming the basis of other music—the assertion that not the melodic relationship, but the proximity of pitch is the psychological condition of the æsthetic effect of such music. I have elsewhere expressed my conviction that the fundamental psychological laws of music are the same all over the world. The modern progress of anthropological investigation has already overthrown many a prejudice concerning differences in the congenital psychological organization of the different races of man. I am convinced that the result will be the same with regard to music, as soon as the theoretical study of music shall have become less superficial and artificial, less speculative and more psychological.

In order to contribute something towards a better understanding of quartertone-music, I made up such music, following the same laws which I had found by analysis of our common music; but with this difference that I did not interrupt the procedure whenever a quartertone resulted, but on the contrary included some quartertones. The problem was to describe the æsthetic effect of this music at the first hearing and later when it had become more and more familiar. The music was a melody plus a harmonization, as seen from the following table.

9	9	35	135	135	75	9	15	27	105	405	27	15	15	35	135	135	15
15	15	15	15	225	15	15	45	45	45	45	45	45	21	15	15	225	45
45	21	5	45	45	45	45	135	135	75	135	135	9	9	5	45	45	75
						15	45	45	15	405	45						

.01 E.S.:	204	63	49	70	112	85	119	204	63	49	70	112	
Symbol:	15	135	35	9	75	5	21	45	405	105	27	225	15

The first line of the above table represents the melody, which I first made up. The following three lines represent the harmonization, which I added to the melody. The last two lines represent (within a single octave) all the tones used, arranged according to pitch. The last line contains the symbols which I have introduced for the theoretical notation of music. The line above the last shows the distance between each succeeding pair of tones of the series, measured in hundredths of an equally tempered semitone (E. S.). I have added this line in order to prevent any reader from attempting to repeat my experiments by playing the above music on the piano. This is impossible, since only a few of the intervals are approximately equal to one or two semitones; most of them are much smaller, and two are almost exactly equal to a quartertone (.49 E. S.).

I played this music on a reed organ, which was perfectly tuned in what I have called the Complete Musical Scale up to a convenient limit. I first played the melody unharmonized to myself. The æsthetic effect (I shall first describe the observations without any attempt to explain them) was highly disagreeable. I then played the melody plus the harmonies. The æsthetic effect was not agreeable, but not by any means so disagreeable as when I played the melody unharmonized. I then played the whole piece a few times every day for a couple of weeks and noticed that gradually it lost all its disagreeableness and became more and more beautiful; at the same time I noticed that the more I committed the music to memory, the better became the æsthetic effect, until, when I expected the tones of each chord before I heard them, no dislike of any of the tones heard remained. I now played again the unharmonized melody and found that the æsthetic impression was very different from the one I had had at the first hearing. (The music was made up entirely by theoretical means, without the use of the ear.) Although there was no particular beauty to speak of, as in the harmonized piece, there was no disagreeableness left. It sounded simply commonplace.

I then experimented on a considerable number of other individuals. I told them that I would play to them some music of an unusual character (but did not tell them any further details) and asked them to answer the following questions:

"Does this music seem to be of a familiar type or does it sound strange? If familiar, tell of what sort of music it reminds you. If strange, tell what distinguishes it from music you are familiar with (1) theoretically, (2) in its emotional effect. Tell, further, whether the first hearing impressed you differently from the last.

The questions were somewhat indefinite, because I did not wish to make any suggestion as to my real problem (the effect of quartertones); consequently some of the answers referred to matters with which we are not concerned. The answers are reported in the following only so far as they refer to the present problem. I played the harmonized piece very slowly a dozen or fifteen times. Fourteen subjects took part in this experiment. In parentheses, after the names of the subjects, their musical specialty, if any, is indicated; subjects from the student body are indicated by the word "student."

Prof. H. B. Almstedt (piano, organ): The chords seem familiar and strange. The emotional effect was pleasing, save in a few instances.

Mr. W. G. Bek (student): I liked the last playing better than the first, very much more so.

Mr. H. Borgstadt (student): Strange. First impression better than last.

Mr. C. C. Crouch (student): The music seemed different from any I am familiar with. At first it seemed unpleasant, at last it became pleasant or at least not unpleasant. The emotion it aroused seemed to be sorrow.

Mr. W. Higbee (student): It sounds like a funeral dirge. It seems to have occasional errors in harmony and has some discords, apparently. Its emotional effect is as a dirge. The effect gets more pleasing as it progresses. The general tone seems to be foreign, like we usually hear on 'Midway' in front of 'Streets of Cairo.' The scale seems different.

Miss L. Hoffman (student): It sounds strange because the successions are so unexpected. When I heard the first chord, it reminded me of something I had heard somewhere, and I had a vague idea of what could follow it, and when it did not, then it sounded strange. I believe I liked it best the last time it was played.

Mrs. C. Jones (voice): Sounds unfamiliar. Some of the chords suggest Grieg in their weird strains. More pleasing at the last than in first hearing.

Mr. R. Kern (student): It sounds strange. The emotional effect is sadness. The chords do not seem to be made up of closely related tones.

Miss C. Kerr (piano): If by type you mean the chord progression, it sounds strange. Suggests a German Choral in minor. It is not our scale. It is weird like a barbaric funeral chant.

Miss C. U. Mills (piano, organ): Is pathetic on account of the predominance of minor.

Mr. H. T. Moore (student): This music is not exactly familiar or entirely strange. Some of the intervals come in in rather unexpected succession. I liked the last better than the first, because I became more used to the succession of chords.

Prof. F. H. Seares: First impression more pleasing, decidedly, than last.

Prof. T. C. Whitmer (piano, organ, composition): It is music not familiar to the average ear. It belongs to the mediæval modes, and therefore it possesses no tonality. The oftener it is played the more beautiful it becomes. We are not accustomed to the limitations in scale, such as is here represented.

Miss A. Zimmerman (student): Sounds strange. It is too weird for church music and not martial either. It sounds like some Arabian music I heard in 'Buffalo Bill's Wild West Show.' The first time was disagreeable because of what seemed like discords. But it becomes much more pleasant in the last till it is fascinating.

To sum up the results, let me say, that of the fourteen subjects eight declare that the æsthetic effect is improved by hearing the music repeatedly. Four do not mention any such difference at all. Only two (not especially musical) subjects declare that the first impression was better.

A fortnight later I repeated the experiment, but now told the subjects that the music contained quartertones, to which I called their attention. Some of the former subjects were not present this time, but there were some new ones.

Mr. W. G. Bek (see above): The whole sounded rather pleasing to me at the first playing, but grew more and more pleasing toward the end. There is something in the music that gives me a sort of *Andachtsstimmung*. It did the other time and does even more so this time.

Mr. H. Borgstadt (see above): The music is not altogether unfamiliar. Some parts sound like some sacred music that I have heard. Some parts sound well, others not so.

Prof. W. G. Brown: The music suits me. I am used to and enjoy the drone of the bagpipe. Not only am I reminded of this, but I am forced to remember some of Wagner's operas, I think parts of *Lohengrin*. I am, however, forcibly impressed by its stately, not to say churchly character. I enjoy such music extraordinarily well.

Mr. D. Burnet (violin; good observer, but generally much inclined

to theorizing while serving as subject): Does not sound so bad at the end as at first. Seems to me, though, that this is because the ear becomes indifferent, and I do not believe I could ever come to enjoy music built on such a scale. Some of the harmonies are not unpleasant and some of the discords not impossible. But some of the intervals sound so impossible that I do not believe they would ever be bearable.

Mr. F. C. Donnell (student): Sounded weird, like a funeral dirge; but instead of sounding bad, it sounded good to me—really pleasing. The more I heard it, the better it sounded.

Miss L. Hoffman (see above): Some parts of it very pretty, other parts weird. Most of the ideas it suggested were pleasant.

Mrs. C. Jones (see above): Better as it grows more familiar.

Miss C. Kerr (see above): It does not sound so objectionable to-day.

Miss C. U. Mills (see above): It seems to be a fact that the example grows more endurable.

Prof. T. C. Whitmer (see above): Undoubtedly the music becomes much more beautiful on acquaintance. The new sensations of the quartertones become most agreeable with frequent repetitions, let me repeat.

Miss A. Zimmerman (see above): The music loses its discordance and gets very pleasant and fascinating. It is so new and different.

Of the eleven subjects eight had been present the other time. Five of them confirm their former observation that the æsthetic effect increases with familiarity. Two who did not express any opinion on this point before make now the same observation. Of two subjects who declared that the first impression was better, one is now absent, the other does not state again that the æsthetic effect is getting worse. The three new subjects agree with the rest in observing that familiarity increases the æsthetic effect.

A fortnight later I made a third experiment, most of the subjects of which had heard the harmonized piece either once or both times before. One only heard this quartertone-music now for the first time. I played alternately the melody accompanied by a single bass tone (15 in the first and third, 45 in the middle part) and unaccompanied. The subjects were requested to compare the æsthetic effect of the one case with that of the other.

Prof. H. B. Almstedt: I prefer the double to the single series. The bass seems to veil the disagreeable less-than-half-tone sequence of the single series.

Mr. W. G. Bek: I do not like the melody as such. In connection with the accompaniment it sounds better.

Mr. H. Borgstadt: When the series is accompanied by another tone it sounds better than when played alone.

Prof. W. G. Brown: Better when accompanied by another tone. It seems out of tune at certain parts when the melody alone is played, which out-of-tune places are more or less pleasantly modified when accompanied.

Mr. F. C. Donnell (had heard it only once before): I like it better when the other tone is not sounded with it. I would not recognize either as being worthy of the name of a tune, although I enjoyed it last week when it was accompanied by full chords.

Mrs. C. Jones: I prefer the combination with other notes, rather than the simple tones.

Mr. R. Kern: I like it very much more with the additional tone. Some of the notes seem out of tune. This is much less noticeable with the extra tone. I rather like the melody with the extra tone.

Miss C. Kerr: I prefer it either alone or fully harmonized. It is *entsetzlich* with the second tone.

Miss C. U. Mills: Some of the intervals are too small to be acceptable. (No definite answer to the question given.)

Dr. Caroline Stewart (new subject): Better alone; alone somewhat out of tune, with a second tone more out of tune.

Prof. T. C. Whitmer: I prefer the melody played with the second melody. Sounded alone, the melody seems more out of tune. Less relationship among the tones is apparent when the melody is played alone.

It is of the utmost importance to notice that those subjects who had come to enjoy the harmonized piece more or less, preferred the melody accompanied by a bass, whereas those who had not arrived at familiarity with the harmonized piece, preferred the melody unaccompanied by a bass.

From my own introspections and those of the other subjects I draw the following conclusions. Tones in the interval of a quartertone are unrelated, but there is no psychological law excluding, for this reason, quartertones from being used musically; in our common music, also, tones are sometimes used which are not directly related. Tones forming the interval of a quartertone may be used musically if both are related to a third tone appearing in the music. That we do not enjoy at once every tone combination with which we are unfamiliar is a well known fact. Even Beethoven's works were not enjoyed at once by every one, but, on the contrary, very severely criti-

cized by some. But the fact that mere familiarity with quarter-tone-music makes it possible for us to enjoy such music, proves that the fundamental laws of quarter-tone-music cannot with any probability be assumed to be different from those of any other sort of music.

The individual differences observed can be explained without any difficulty. 1. Some individuals by their congenital mental constitution like music which is atonic and made up of less closely related tones better than other individuals do. There were some subjects who liked the quarter-tone-music almost from the start. 2. Some individuals have acquired a strong habit of thinking musically, and of course in music with which they are familiar. Quarter-tone-music must naturally be 'out of tune' to them, because it overthrows all their firmly established habits of musical thought; and it takes them a long time to establish new habits of musical thought, including these new combinations of tones.

The melody used in the above experiments is one which is not very readily appreciated. It is not very melodious. The first tone 35 is (if unaccompanied) very far away from any related tone (the first one is 15). If we add to it a bass which is related to 35, we should expect the tone 35 to exert more readily its æsthetic capabilities. Most individuals, therefore, preferred the melody when accompanied by a bass, to the same when played alone. But why did a few still prefer the melody unaccompanied? It is not difficult to explain these exceptions to the rule. If, for any reason, we *expect* some other combinations of tones than we hear, we most readily perceive the tones simply as *mistuned* combinations of the sort we expect. It is by no means impossible to be æsthetically affected by a piece we are familiar with, if it is played on a very mistuned instrument. But if now the bass is added, this acts as a strong factor, compelling the hearer to perceive the relationships as they actually are. Then this music is '*entsetzlich*' because it violates the habit of the hearer who cannot help expecting tone combinations of another sort. This explains very well the exceptions which we find among the records.

Let me state again, from another point of view, the causes why quarter-tone-music is not usually at once appreciated. 1.

We *do not like* it (we are neutral), because, the music being new to us, the relationships are not sufficiently effective. 2. We *dislike* it, because, from our musical habits, we expect something else. But, of course, occupation with something indifferent turns after a few moments into positive dislike; *i. e.*, dislike is the final result of either condition.

I should like to know, then, the facts compelling us to assume that the quartertone-music of oriental peoples is based on psychological laws fundamentally different from the laws of our common music! If we can make up quartertone-music by merely applying the laws of our common music, and if the conditions of our appreciation of this quartertone-music are such as above described, is it not highly probable that the same laws can be applied also to that quartertone-music which we actually find among foreign nations?

EIN BEITRAG ZUR EXPERIMENTELLEN AESTHETIK.

Von Professor O. KUELPE, Würzburg.

Seit einigen Jahren benutzte ich zur Demonstration psychologischer Apparate und Versuchsanordnungen, sowie zu Vorlesungsexperimenten einen Projectionsapparat. Auf Pauspapier werden die Figuren aufgezeichnet (auch Glasplatten mit Canada-Balsam präparirt dienen dem gleichen Zweck), sofern nicht Diapositive zur Verfügung stehen. Aber auch selbständige Experimentaluntersuchungen können auf diese Weise ausgeführt werden. Dabei ist es für manche Zwecke vortheilhaft, das Bild nur eine bestimmte kurze Zeit zu enthüllen. In solchen Fällen hat sich das einfache Aufsetzen eines photographischen Momentverschlusses sehr bewährt. So ist eine längere Versuchsreihe über die Abstraction entstanden, die ich in Verein mit Professor Bryan ausgeführt, aber leider noch nicht habe veröffentlichen können.

Auch für gewisse ästhetische Probleme schien sich diese Versuchsanordnung zu empfehlen. Ich habe daher, um wenigstens vorläufige Erfahrungen auf diesem Gebiet zu sammeln, an drei Vp je 28 ästhetische Experimente in folgender Weise angestellt. Im Dunkelzimmer des psychologischen Instituts, wo der Projectionsapparat sich befand (Schuckert'sche Bogenlampe, Bildträger, Linsen und optische Bank von Zeiss), sass die Vp 4 m. vor dem die Schmalwand des Zimmers deckenden weissen Schirm. Die lineare Vergrösserung betrug das 22-fache, die gesammte Bildöffnung hatte 1.5 m. im Durchmesser. 28 Diapositive¹ wurden mir von dem Leiter des Würzburger kunstgeschichtlichen Museums, Herrn Professor Wolters, freundlichst zur Verfügung gestellt. Sie wurden mit Rücksicht auf eine gewisse Mannichfaltigkeit ästhetischer Bethätigung ausgewählt und sollten zugleich im

¹Geliefert von Dr. Stoedtner, Berlin N. W. 21.

Allgemeinen nicht dem alltäglichsten kunsthistorischen Wissen zugänglich sein. Es waren die folgenden:

1	Pergamon, Altarrelief. Gigantomachie: Zeusgruppe (Ergänzung)	20 ¹
2	Pergamon, " " Athenagruppe (Ergänzung)	3
3	Pergamon, " " Hekate, Artemis (Ergänzung)	28
4	Auf's Knie gesunkener Gallier (Venedig)	21
5	Auf den Rücken gesunkener Gallier (Venedig)	25
6	Parthenon. Ecke reconstruiert	24
7	Meleager (Berlin)	19
8	Niobe und Tochter (Florenz)	2
9	Fliehende Niobide (Florenz)	9
10	Söhne der Niobe (Florenz)	17
11	Zweiter Sohn der Niobe (Florenz)	13
12	Ares Ludovisi	27
13	Krieger von Delos (Torso)	11
14	Schleifer (Florenz)	23
15	Parthenon von Osten	7
16	Komposit-Kapitäl (Neapel)	16
17	Dorische Säule mit Gebälk. Farbig	6
18	Ionische " " " "	1
19	Korinthische " " "	4
20	Dorische Säulen im Grundriss nach Perrot-Chipiz	26
21	Dorische, ionische, korinthische Säulen im Grundriss	22
22	Tempel von Korinth, Reste, mit Landschaft	12
23	Tempel von Paestum	5
24	Tuskanischer Tempel, Reconstruction	10
25	Theseion (Athen)	18
26	Nereidenmonument von Xanthos, Reconstruction	14
27	Porta nigra (Trier). Innenseite	8
28	" " " Aussenseite	15

Die Expositionsdauer betrug in allen Fällen 3 Sekunden und wurde mit Hilfe eines am Schlauch eines gewöhnlichen Rouleauxverschlusses angebrachten Thornton-Pickard-Hahns mit ausreichender Genauigkeit eingestellt. Ein Jetzt! etwa 2 Sec. vor der Enthüllung des Bildes bereite die *Vp* vor. Ein Fixationspunkt war ihr auf dem Schirm bezeichnet, damit sie immer von derselben Stelle aus ihre Beobachtung beginne.

¹Die rechts beigesetzten Ziffern bezeichnen die Stellung in der Reihe der Versuche, wie sie unten als "Bildnummer" in der Tabelle aufgeführt ist.

Doch durfte sie hernach den Blick beliebig über das Bild wandern lassen. Die jeder *Vp* vor Beginn der eigentlichen Versuche mitgetheilte Instruction bestand darin, dass sie 3 Sec. lang ein ihr gezeigtes Kunstobject aufmerksam, aber in möglichst passiver Hingabe an das Bild, betrachten und mir darnach so treu und vollständig als möglich Auskunft darüber zu geben habe, ob es ihr gefallen oder missfallen habe oder indifferent gewesen sei, worauf sich die Gefühlsreaction gerichtet und was sie besonders bemerkt bzw. wahrgenommen habe. Diese Aeusserungen wurden von mir genau protokolliert und durch einige Fragen event. ergänzt.

Als Versuchspersonen dienten mir die Herren Privatdocent Dr. E. Dürr, H. A. Abbott, Lecturer in Toronto, und Dr. A. Scheunert, denen ich auch an dieser Stelle meinen besten Dank ausspreche. Ich bezeichne sie im Folgenden willkürlich mit den Buchstaben *A*, *B* und *C*.

Ich theile im Folgenden zunächst die Protokolle der 3 *Vp* in paralleler Anordnung mit.

BILD-NUMMER. VP. A.

1. Gefällt. Lebendige Bewegung zweier Pferde, deren Contouren gut gezeichnet sind. Auf ihnen zwei möglicherweise festende Reiter. Die Farben, orange bis roth-orange, vielleicht auch etwas violett-purpur, sind indiffernet. Die Situation wird nicht nachempfunden. Vielleicht zwei Säulen.

2. Gefällt. Drückt die Gefühle (Hoffnungslosigkeit) gut aus. Die Kinder scheinen etwas zu wünschen, was die Mutter nicht erfüllen kann. Schattirung und Contouren angenehm. Keine Mitempfindung. Vorstellung einer Marmorfigur in einem Teich.

VP. B.

Indifferent. Nicht recht klar geworden aus dem Ganzen. Leises Lustgefühl bei Erkenntnis der Pferde. Sodann Wortvorstellung: griechischer Fries, linke Ecke eines Daches. Erscheint farblos. An den Pferden ist besonders der gestreckte Hals aufgefallen.

Erinnerung an einen Abguss im Leipziger Museum, womit Lustgefühl gebunden. Die Figur gefällt nicht sonderlich, die Augen missfallensogar. Richtung der Gedanken auf eine Besprechung der Figur in einem kunstgeschichtlichen Seminar, wo namentlich auf den Faltenwurf eingegangen wurde. Der Schmerz kommt ziemlich grell zum Ausdruck, das Gesicht ist derb und fleischig erschienen. Das Ganze nicht sympathisch.

Schön: Erinnerung an die germanischen Sagen mit starker Lust im Gegensatz zu den griechischen verbunden. Klare Vorstellung vom Sinn nicht vorhanden; zuerst an Walküren,

VP. C.

Gefällt. Bemerkt zunächst dass eine Perceptionszeit vorhanden ist. Die einzelnen Figuren nicht deutlich gesehen. Besonders angenehm das unten rechts eingesetzte blaue Stück. Wortvorstellung: griechischer Tempel.

Indifferent, weniger gefällig als das vorige. Kurze Zeit nach dem Namen "Niobe" gesucht. Augenaufschlag und Gesicht besonders beachtet. Wortvorstellung: bekannte Figur. Das Werk niemals besonders geschätzt.

Gefällt. Ueberlegung, ob ein antikes Marmorwerk oder eine Bronzeskulptur der Renaissancezeit. Eine Nike, deren Stellung an ähnliche Figuren auf modernen Kriegerdenk-

3. Gefällt: war aber zu complex um verstanden zu werden. Drei Figuren gesehen, von denen zwei deutlich, Frauengestalten. Contouren und Stellung gut.

dann an eine Siegesmedaille gedacht. Der Eindruck eines viel bedeutenden Werkes. Am klarsten 2 weibliche Gestalten mit Flügeln. Es müsste interessant sein, sich weiter in das Bild vertiefen zu können. Eine harmonische, abgerundete, weiche Gruppe, in der Gefühl liegt und die den Eindruck eines Ganzen macht.

4. Gefällt. Säule und Ecke eines Gebäudes. Die griechische Technik tritt gut hervor, die Contouren sind für ein Gebäude gut, sonst zu geradlinig. Das Capitäl nicht genau gesehen, wohl korinthisch. Erst wurde das Gebäude, dann die Ecke vorgestellt.

5. Als historisches Bild gefällt es, sonst indifferent. In der Mitte ein grosses Gebäude mit 8 Säulen, links eine Ruine. Die Säulen alt, nicht mehr ganz intact. Sofern bloss die Ordnung der Säulen und die Verhält-

mälern erinnert. Ausserdem eine Frauengestalt in ähnlicher Haltung und eine in sitzender Stellung. An der Nike eine vorwärtsschiebende Bewegung beobachtet und dabei selbst eine entsprechende Bewegungsempfindung (wie beim Ausstrecken des rechten Arms und Vorstrecken des Körpers) gehabt. Das Ganze hatte eine Abgeschlossenheit und Einheitlichkeit, die gefiel.

Indifferent. Prächtiges Korinthisches Säulen-Capitäl, spätromisch (etwa aus der Zeit des Caracalla). Stück eines Tempels. Eine gelbe Schattirung unten beim Uebergang vom Dunkeln in das Bild war besonders fesselnd und gefällig.

Gefällt: Empfindung der Weite, eines Fernblicks, Eindruck der Massigkeit, Schwere, Solidität, Dauerhaftigkeit merklich. Gedanke an den Poseidontempel von Paestum.

BILD-NUMMER. VP. A.

nisse der einzelnen Theile betrachtet werden, gefällt das Gebäude.

VP. B.

nicht so gedrückt, wie aus der Ferne gesehen, sein mögen. Das Dach kommt dem Boden zu nahe. Warum stehen die Tempel so dicht gedrängt? Die Aufmerksamkeit auf den Rechten concentrirt. Die Regelmässigkeit wurde bemerkt, blieb aber indifferent. Eigenartiges Vorstellungsgefühl: Wo ist der Nutzraum der Gebäude?

6. Gefällt. Säule mit Architrav, kreisförmige Figur. Hat als Ecke von einem Gebäude sehr gefällige Contouren, gute und wie es scheint reiche Arbeit. Farben roth oder orange, vielleicht auch violett oder purpur. Die Figur in der Mitte ein Pferd? Die Vorstellung des Zwecks ist sehr wichtig für die ästhetische Beurtheilung.

7. Gefällt sehr. Ordnung, Regelmässigkeit, Proportion und Kannelirung der Säulen; ferner der antike Charakter der Ruine und die Schattirung.

8. Gefällt weniger, nur als Ruine

VP. C.

Sehr gefallen. Dorische Giebelecke. Einzelne bläuliche Farbentöne besonders schön. Die doppelte griechische Kante und das Medaillon auf dem Architrav gefallen sehr. Das Ganze sehr einheitlich und harmonisch. *Vp* hätte es sich gern noch länger angesehen. Sie liebt überhaupt Farben.

Indifferent. Eindruck des Verworrenen, Uneinheitlichen, einer unerfreulichen Verwüstung. Die Säulen des zerfallenen Tempels waren ein Ruhepunkt.

Etwas gefälliger. Die Unregelmäs-

BILD-NUMMER. VP. A.

und als ein Bau der viel Arbeit erfordert hat. Die gerade viereckige Form missfällt. Der Sinn des Gebäudes wurde nicht verstanden.

9. Gefällt. Lebendig, Contouren sehr gefällig, aber die Stellung nicht ganz klar. Die linke Hand nicht verstanden. Tendenz zu Organempfindungen bemerkt, welche die Vorstellung des Werfens begleiten. Gesicht und Ausdruck sehr angenehm.

10. Gefallen hat die grosse Ordnung und Regelmässigkeit, aber die Proportion scheint nicht gelungen, d. h., es müssten mehr Säulen sein. Die Treppen haben ebenfalls nicht recht gefallen. Die Figuren am Giebel nicht genau verstanden, scheinen im Verhältniss zu gross und überflüssig, ohne Bedeutung für das Gebäude.

11. Die Contouren gefallen. Die Haltung der Figuren hat die Vorstel-

VP. B.

das Heidelberger Schloss stark lustbetont. Aber das H. Schloss ist weniger massiv und schöner durch seine reiche Vegetation. So eine Kaserne würde nicht gefallen, wenn sie nicht an das H. Schloss erinnerte. Das dunkle Grauerschien angemessen.

Zuerst an Niobe, dann an eine Niobide gedacht. Gefälliger als (2); schlankere Figur, gefälligere Linie des Mantels, kein so überstarker Affekt.

Angenehme Ueberraschung. Wieder ein Fernblick. Aber Eindruck einer schlechten Reproduction, namentlich der Athene auf dem Dach. Gedanke an das Parthenon. Die Dunkelheit des Innern sticht gut ab gegen die Helligkeit des Vordergrundes, wodurch Vorstellung eines Raumes geweckt wurde. Das Haus schien zu niedrig.

Lebhaftes Lustgefühl an den scharfen Contouren und an der Schattirung.

VP. C.

sigkeit des Baues fiel zunächst auf. Der Blick durch die Thorbogen war erfreulich; der Eindruck des Lebens knüpft sich daran. Wortvorstellung: Rom.

Indifferent. Dachte an Niobe, sah aber sofort dass es etwas anderes war. Göttin der Schnitter? Offenheit und Frische eines Naturmädchens. Das Zurücklangen des rechten Armes hat etwas Manierirtes.

Recht gefällig. Momentaner Gedanke an die Nationalgalerie in Berlin. Die weite Oeffnung zwischen den Säulen in der Mitte, die Säulenthelle und die Säulenordnung ebenso wie die Treppe gefielen sehr. Die Aufsätze erschienen zu flatterhaft und unruhig. Eindruck des weiten, feierlichen, ernstesten. Dabei nichts von Organempfindungen gespürt.

Indifferent. Perceptionszeit deutlich bemerkt. Die Wortvorstellung

BILD-NUMMER. Vp. A.

lung von Stärke reproducirt und in soweit gefallen. Der Torso als solcher missfällig. Der Kopf ist zum Verständniss erforderlich. Die Schattirung sehr angenehm. In der Umgebung war noch etwas, was *Vp* nicht bezeichnen kann. Organempfindungen im rechten Arm gespürt, als wenn *Vp* die Stellung der Figur einnehmen wolle.

12. Ordnung und Proportionalität der Säulen gefällt, noch mehr die Kannelirung und Schattirung. Die Landschaft war indifferent, hat aber nicht gestört.

13. Die Figur rechts sehr gefällig, drückt die Haltung eines furchtlosen Vertheidigers aus; links vielleicht der Angreifer, wenn beide überhaupt etwas mit einander zu thun haben. Sehr lebendige Stellung. *Vp* würde Organempfindungen gehabt haben, wenn nur eine Figur dagewesen wäre. Die Verschiedenheit der beiden hinderte deren Entwicklung.

Vp. B.

Aber nur der Torso von einem gestürzten Krieger, also ein blosses Fragment. *Vp* empfindet ein intellectuelles Unbehagen darüber.

Das einzige Gefallen beruht auf der Landschaft: endlich einmal Natur und Berge. Die Ruine zu einfürmig, und die Berge zu gleichförmig.

Gefallen an dem plastischen Eindruck, an der Vorstellung, dass es ganze Figuren sind, an der straffen Körperhaltung besonders der rechten. Gedanke an Bestandtheile der Niobidengruppe. Organempfindungen in der Schenkelbeuge. Der Ausdruck blieb unklar.

Vp. C.

„Museum“ hat sich eingestellt. Nicht klar, wie der Torso zu reconstituiren wäre. Auch zwei andere Stücke gesehen. Schwache Bewegungsempfindungen, wie wenn die Extremitäten auseinander gespreizt würden.

Indifferent. Anfangs gefiel der Eindruck grosser Helligkeit, wie wenn die Sonne die Landschaft beschiene. Trotz der Oede, wohlthuender Eindruck der Natur. Die Trümmer selbst machen den Eindruck des Gewirrs.

Indifferent. Die Figur links hat eine missfällige, manierirte, unnatürliche Bewegung. Die andere macht den Eindruck eines Mannes, der etwas lebendig explicirt.

BILD-NUMMER. VP. A.

14. Schattirung nicht angenehm, zu hell. Das Gebäude scheint zu hoch auf einer viereckigen Säule, was missfällt. Die Ordnung der Säulen und das Gebäude für sich gefallen. Die Unmöglichkeit einer Zweckvorstellung ist sehr störend.

15. Nicht so angenehm. Die Regelmässigkeit des Ganzen gefiel, aber die der zahlreichen viereckigen Glieder des Baues missfiel. Wortvorstellung: Heidelberg.

16. Die Contouren, die Ordnung der Theile, die Schattirung, die Harmonie der Anlage, auch die Arbeit gefallen sehr. Von dem Zweck und dem Ganzen, zu dem dies Säulenkapital gehört, hat $\frac{1}{p}$ eine nur sehr geringe Vorstellung.

17. Die Figur im Ganzen gefällt,

VP. B.

Zuerst leises Gefallen, dann aber starkes Missfallen: ein unmöglich hoher kubischer Unterbau für den Tempel, dessen Sockel nicht vorragt. Die Figuren sehr undeutlich, stark gehäuft. Immerhin etwas Ganzes und Hochtragendes.

Heidelberg Schloss in verschönerter Auflage. Die reiche Gliederung der Gesamtform gefiel, aber sie erschien mehr verwittet und ruinenhaft.

Schön, aber zu massig. Die Säule mit dem Akanthusblatt gefiel. Die klotzigen Mauern dahinter erdrücken die Wirkung der Säule. Eindruck von etwas wuchtigem und damit verbundene undefinierbare Organempfindungen. Vorstellung dass die Säule bloss zur Verzierung nicht zum Tragen da sei.

Interessirt sehr und gefällt etwas,

VP. C.

Indifferent. Die Schattirung zu hell. Die Figuren zum Theil zertrümmert und dadurch unerfreulicher Eindruck von etwas Verwüstem, Zerfallenem. Glaubt etwas ähnliches schon gesehen zu haben. Das Verhältniss zwischen Ober und Unterbau gefällt.

Sehr fesselnd. Das vorhin (8) gesehene Gebäude von der Rückseite. Vorn zwei kleine Bosquets, die belebend wirkten. Die Architektur sehr schön, schwer, prächtig, im römischen Stil der Kaiserzeit, der $\frac{1}{p}$ in seiner Würde immer sehr sympathisch gewesen ist.

Die Beleuchtung gefällt sehr. Das Licht von rechts lässt die Ornamente in ihrer Tiefe sehr gut hervortreten. Vorstellung eines Interieurs, als ob dies korinthisch-römische Kapitäl sich im Innern eines riesigen, sehr hohen Raumes befindet.

Indifferent. Die Schulterbewegung

BILD-NUMMER. VP. A.

aber die Stellung scheint eine blosse Pose, zwecklos zu sein, und missfällt darum. Schattirung und Contouren sind schön. Besonders missfällig die Stellung des Kopfes. Tendenz zu nachahmenden Bewegungen empfinden.

18. Gefällt im Ganzen, hat aber zu viel Säulen und im Verhältniss zu enge Oeffnungen. Der Vordergrund missfällt.

19. Gefällt nicht besonders. Der Kopf scheint zu klein. Die Haltung zu ruhig, hat zu wenig Bedeutung. Der Körper gefällt, und der Speer erweckt die Vorstellung eines Fechters.

20. Eine Figur links gefällt, weil sie die Vorstellung der Tapferkeit erweckt, während die zwei Figuren rechts missfallen, da sie feige zu sein scheinen. Unbestimmte Tendenz zu

VP. B.

weil es ein Ganzes ist und eine gewisse Bedeutung zu haben scheint. Auch die Breite und Kraft des Körpers gefallen. Damit sind leise Empfindungen im Brustkorb verbunden. Die Undeutlichkeit des Gesichtsausdrucks missfällt.

Die Lage des Tempels auf einer Anhöhe und seine Einsamkeit machen einen gefälligen Eindruck.

Schön der plastische Eindruck und die kraftvolle Ruhe in der Haltung. Ein eigenthümliches Organgefühl des Gutmüthig-Kraftvollen. Etwas unklar die Bedeutung des Speers.

Zuerst leiser Anklang an das Germanische, dann aber als griechisch erkannt. Erscheint als Kampf der Götter und Giganten. Der Zeus gefällt. Sein weites Ausholen mit dem

VP. C.

erscheint ganz unnatürlich, überaus thöricht: der Körper sonst sehr schön, ebenmässig, weich und elastisch. Spürt eine Druckempfindung, wie beim Anfassen eines solchen Körpers. Das Gesicht macht den Eindruck eines trinkenden Vogels.

Indifferent. Es wirkte befremdend, dass der Tempel eine schiefe Stellung zu dem Beschauer hat. Der Bau erschien dadurch verzogen. Die Kakteen im Vordergrunde interessirten.

Gefällt trotz der ungünstigen Beleuchtung von links hinten. Erinnert an eine Grabstelle. Das Gesicht auf dem kurzen Hals lässt an Apollon denken, ist aber dicker, und der geöffnete Mund störte etwas. Der äusserst lebendige, wie schnellathmende Hund gefiel sehr. Ein Jäger, der sich an seinen Speer anlehnt. Das Ganze schien bekannt.

Indifferent. Die Einheitlichkeit des Ganzen wird gestört durch die zuweit herausragenden Arme von Zeus auf der einen Seite und einem Titanen (?) auf der anderen Seite. Organ-

BILD-NUMMER. VP. A.

Organempfindungen, aber das Bild zu complex.

21. Die Stellung und die Figur gefallen, ihre Bedeutung (Vorstellung von einem Gladiator, dessen Lage nicht motivirt erscheint) missfällt.

22. Die Säule links gefällt am meisten, mit ihrer Kannelirung und der Einfachheit des Kapitäls. Die anderen Säulen waren zu klein.

23. Gefällt nicht sehr. Das Gesicht hässlich, die Bedeutung der Haltung unklar. Der Körper selbst nicht unangenehm. Eindruck einer dreieckigen Form. Unbestimmte Tendenz zu Organempfindungen.

24. Nicht hässlich, aber auch nicht besonders gefällig. Die Schattirung

VP. B.

Arm erweckt Organempfindungen in der rechten Schulter. Die Symmetrie und Abrundung der Gruppe gefiel, aber eine klare Vorstellung ihrer Bedeutung war nicht vorhanden.

Nicht schön. Die Haltung steif, das Gesicht in seiner Ausdruckslosigkeit lächerlich. Vorstellung eines Barbaren, auch auf Grund der Kleidung gebildet.

Sehr unschön. Wie eine unfertige Zeichnung, die eine Tendenz sie zu vervollständigen erweckt. Eine Schölerarbeit, ohne Plastik.

Sehr hässliches, fleischiges, plumbedummes Gesicht. Lebhaftes Erwachung an einen Frosch. Die Haltung nicht recht deutlich geworden.

Wieder Eindruck einer unfertigen Zeichnung, aber gefälliger insofern

VP. C.

empfindungen, wie beim Zurücklehnen des Armes, das für die Kampfstellung etwas zu gemüthlich erscheint.

Indifferent. Die Armbewegung nach oben, wie ein Stechen, wurde empfunden. Die Pose des niedergestürzten Kriegers, der sich währt, erschien theatralisch, glatt, sehr idealisirt, das Gesicht schablonenhaft.

Gänzlich indifferent. Kein ästhetisches Verhalten. Eindruck einer Handzeichnung, nicht von wirklichen Säulen. Reproduction der Namen: dorisch, ionisch, korinthisch.

Wirkte zunächst fast komisch. Das Gesicht machte den Eindruck als wenn ein Frosch imitirt und groteske Töne ausgestossen würden. Organempfindung des Blickens in die Höhe. Unästhetische aber äusserst natürliche Pose eines Mannes, der Getreide zu sieben scheint.

Eindruck einer blossen Zeichnung, und dann einer Gebäudecke, an die

BILD-NUMMER. VP. A.

zu regelmässig. Die Säulen schienen nicht parallel zu sein. Die Einfachheit gefiel.

VP. B.

wenigstens ein Theil einigermassen parallel war.

VP. C.

sich eine sehr lange Seitenfront anschliesst. Mit dieser Vorstellung war eine angenehme Empfindung verbunden.

25. Die Stellung hässlich, die Association eines Mitleid Erflehenden unangenehm. Der Parallelismus zwischen dem rechten Arm und Bein missfiel.

Erweckt Heiterkeit. Vorstellung einer Bacchantin in einem Rokokogarten. Die Haltung erschien unmöglich. Leise Empfindung derselben im Rücken localisirt.

Indifferent, fast hässlich. Sehr un-schöne Stellung. Eindruck der Wackligkeit, gezwungene Lage, in der solche Hebung des Kopfes kaum ausführbar ist. Vorstellung eines verfolgten, niedergeworfenen Ringkämpfers.

26. Die Säulen gefallen nicht sehr, convergiren zu stark nach oben. Die Mittlere gefällt noch relativ am meisten.

Eindruck einer unfertigen Zeichnung, die als solche misfällt.

Eine Zeichnung, die gar nicht ästhetisch wirkt. Die Säule rechts sieht aus wie ein altes Krinolinengestell.

27. Im ganzen gefällig. Namentlich die Stellung und die Proportion; der Kopf etwas klein. Die Haltung sehr natürlich, nicht als wenn sie lediglich für das Bild eingenommen wäre.

Schön. Der Mann scheint zu denken. Ruhige, kraftvolle Haltung. Einsamkeit und Form des Körpers gefallen.

Gefällt, aber nicht besonders. Eindruck grosser Ruhe und stiller Betrachtung. Das Ganze sehr harmonisch.

28. Gefällt im Ganzen. Die einzelnen Figuren passen zusammen. Die Schattirung scheint nicht gut, von der Bedeutung hat *l/p* keine Ahnung. Spur von Organempfindungen im Arm beim Anblick der einen Figur, wie bei einer Nachahmung ihrer Bewegung.

Hübsch, obwohl nicht abgerundet. Die Vertheilung von Licht und Schatten und die Lebendigkeit der Scene gefallen besonders. Erinnerung an den Parthenonfries. Die rechte Figur hat nach Form und Haltung besonders gefallen. Die Bedeutung des Ganzen ist nicht klar geworden.

Indifferent. Die anstürmende Amazone rechts hat besonders gefesselt, und Organempfindungen, wie vom Heben des Unterarms und dem Anziehen der Kniee, erweckt. Die Technik scheint gut.

Ueerblicken wir die in der vorstehenden Uebersicht mitgetheilten Aussagen der *Vp*, so finden wir, auch abgesehen von blossen Erkenntnissurtheilen, eine für die kurze Dauer der Exposition bemerkenswerthe Mannichfaltigkeit von Reaktionen. Wir stehen nicht an, von einem ästhetischen Verhalten dabei zu reden, weil die Aufgabe, die den *Vp* gestellt war, dazu aufforderte, und weil die ausgewählten Objekte den allgemeinen Charakter ästhetischer Gegenstände an sich trugen. Allerdings fehlt hier völlig das, was Lipps als das Wesen des ästhetischen Genusses bezeichnet, nämlich die volle oder sympathische Einfühlung. "Die ästhetische Sympathie ist dasjenige, was in allem ästhetischen Genuss die Grundbedingung ausmacht. Sie ist dasjenige, was diesen Genuss, wie immer er sonst geschaffen sein mag, zum ästhetischen stempelt."¹ Ich entnehme jedoch aus unseren Beobachtungen das Recht zu einem anderen, umfassenderen Begriff des ästhetischen Genusses.

Im Uebrigen, fehlt kaum ein für wesentlich gehaltenes Merkmal des ästhetischen Verhaltens. Da ist zunächst der *sinnliche* Charakter seines Gegenstandes. Die Farben und Helligkeiten spielen eine Rolle. Da nur zwei von den gebrauchten Diapositiven farbig waren, so tritt natürlich dieser *materiale* Bestandtheil des direkten Faktors zurück. Doch verdient der individuelle Unterschied hervorgehoben zu werden, der in dieser Richtung zwischen den drei *Vp* bestand. *A* bemerkte zwar die Polychromie, verhielt sich aber indifferent dagegen; *B* ist sie ganz entgangen; und *C* hat sie besonders gefallen. Die Helligkeiten haben nur in ihrem Verhältniss eine Bedeutung für das ästhetische Verhalten gehabt, und auch hier kann man zweifelhaft sein, inwiefern sie lediglich als Bestandtheile des direkten Faktors in Betracht gekommen sind. Wenn eine Schattirung gut erscheint, so ist daran wohl auch ein technischer Gesichtspunkt oder die Frage nach der Beziehung zu dem dargestellten Inhalt gelegentlich betheiligt gewesen.

Viel stärker und in offenkundiger Selbständigkeit treten uns die *formalen* Bestandtheile des direkten Faktors entgegen. "Form, Ordnung, Verhältnisse und Proportionen, Kanneli-

¹*Philos. Abh. dem Andenken Rudolf Hayms gewidmet*, 1902, 369.

rung, Regelmässigkeit, Symmetrie, Ebenmass und Harmonie'' weisen mehr oder weniger ausschliesslich auf ein Gefallen oder Missfallen aus dieser Quelle hin. Gewiss ist auch hier nicht selten eine Unterordnung unter ein "Ideelles" massgebend gewesen, so z. B. wenn ein Parallelismus zwischen zwei Säulen, die dasselbe Dach tragen, vermisst wird. Aber daneben gibt es eine grössere Zahl unzweifelhaft reiner Fälle (besonders bei *Vp A*), in denen die räumliche Gestaltung als solche gefällig gewirkt hat.

Ausserordentlich reichhaltig ist sodann die Ausbeute in Bezug auf die *Bedeutung*, den Sinn, den geistigen Inhalt, die reproduktiven Faktoren des ästhetischen Gegenstandes und auf deren Beziehung zu seinem sinnlichen Charakter. Es ist das um so auffälliger, als ein volles Verständniss des dargestellten Gegenstandes während der kurzen Exposition sich vielfach nicht entwickeln konnte, oder als die nöthigen kunsthistorischen Vorkenntnisse, die zu einem ausreichenden Einblick in den Zusammenhang des Ganzen gehört hätten, nicht in Bereitschaft waren. Da ist in erster Linie auf den Werth aufmerksam zu machen, den der *einheitliche*, abgeschlossene oder der fragmentarische Charakter des Eindrucks für die ästhetische Würdigung hat. Namentlich für die *Vp B* hat dieser Gesichtspunkt eine entschiedene Bedeutung gehabt. Das Ganze, das sie anerkennend hervorhebt, ist immer ein Ganzes dem Sinne nach gewesen, so wenn sie das Bruchstück eines Tempels seiner Gesamtheit entgegensetzt oder mit *A* den Torso als solchen missfällig findet. Ich habe früher von einem Princip der Einheit und Abstufung des Interesses gesprochen,¹ das für den Aufbau des ästhetischen Objektes und für die Intensität des ästhetischen Verhaltens wichtig ist. Theilweise gehört das Gefallen an einem Ganzen zu dem Inhalt dieses Principes, nämlich insoweit ein in sich abgeschlossener Eindruck vorliegt oder vermisst wird, an dem unsere Aufmerksamkeit in monarchischer Unterordnung theilhaftig ist. Ferner möchte ich hierzu noch das Gefallen an einer gewissen *Mannichfaltigkeit* des Inhalts rechnen, da dasselbe gleichfalls mit dem Interesse und der Aufmerksamkeit zusammenhängt. Dieser letztere Gesichtspunkt ist freilich nur bei den *Vp B*

¹ *Vjs. f. wiss. Philos.*, xxiii., 170 ff.

und *C* zur Geltung gekommen. Bei der geringen Dauer der Exposition waren die Bilder zumeist ausreichend, um die Aufmerksamkeit zu beschäftigen, und so findet sich denn auch nur ein paar Mal die Bemerkung, dass etwas zu einförmig oder gleichförmig gewesen sei. In diesen Fällen liegt zudem wahrscheinlich nicht eine Beziehung zum Interesse der *Vp*, als vielmehr ein neues Moment vor, das ich unter dem Namen eines Principis der *Zusammengehörigkeit* und eines associativen Faktors im engeren und eigentlichen Sinne des Wortes kürzlich geschildert habe.¹ Dieses Princip, das sich nicht nur auf die einzelnen Bestandtheile des geistigen Inhaltes, sondern auch auf die Beziehung des sinnlichen Faktors zu demselben erstreckt, ist ein wichtigstes Kriterium für die ästhetische Anerkennung oder Verwerfung bei unseren Beobachtungen gewesen. Eine entsprechende Form, ein guter Ausdruck, die Zweckmässigkeit, die Harmonie, zu grosse Helligkeit, zu gemüthliche Stellung, zu starkes Konvergiren der Säule, unmotivirte Lage, unmögliche Haltung, Einfachheit und Ueberflüssigkeit, Natürlichkeit und Unnatur — alle diese und ähnliche Ausdrücke weisen auf die Geltung dieses Principis hin. Bei allen *Vp* hat es sich wirksam erwiesen.

Wenden wir uns nun zu dem Sinn des ästhetischen Eindrucks selbst, so verdient zuerst erwähnt zu werden, dass die *Erkennbarkeit* oder *Unerkennbarkeit* der Idee, des Gegenstandes, des Zusammenhangs für die *Vp* *A* und *B* an sich eine Quelle der Lust bzw. Unlust gewesen ist. Das ist diejenige Quelle des ästhetischen Genusses, welche Aristoteles in seiner Poetik an dem Beispiel des Leichnams oder widerwärtiger Thiere, die wir in Abbildungen sehen, erläutert. Trotzdem die Bedingungen für die Erkenntniss des Gegenstandes bei unseren Versuchen nicht sonderlich günstig waren, hat dies Moment nur eine ganz geringe Rolle gespielt. Ferner hat in einigen Fällen ein ausgesprochenes Gefallen der Umstand erweckt, dass der Gegenstand *überhaupt ausdrucksvoll*, als vielbedeutend erschien. Vielleicht dürfen wir diese bei den *Vp* *A* und *B* hervorgetretene Aussage zum Interesse in Beziehung bringen. Das Bedeutende, Ausdrucksvolle ruft ein stärkeres und tieferes Interesse hervor, als das relativ Sinn- und Ausdruckslose.

¹ Götting. gel. Anz., 1902, 909 f.

Von grossem Gewicht sind nun endlich innerhalb des ideellen Gegenstandes die *Vorstellungen angenehmer oder unangenehmer Art* gewesen, die durch die Erscheinung des Bildes in der *Vp* geweckt worden sind. Sie umfassen das Gebiet des Fechner'schen Associationsprincips und sind zum Theil auf den Prozess der von Lipps so genannten einfachen Einfühlung zurückzuführen. Für *Vp A* ist z. B. der antike Charakter eines Gebäudes an sich eine Empfehlung desselben. Feigheit in dem Verhalten eines Kämpfers macht ihn ihr missfällig, Tapferkeit, Furchtlosigkeit lassen ihn in einem günstigen Lichte erscheinen. Bei *C* ist der Eindruck einer sonnigen Landschaft, eines offenen und frischen Naturmädchens, eines elastischen Leibes u. A. von erfreulicher Wirkung. *B* fühlt sich durch die Erinnerung an das Heidelberger Schloss und an die germanische Sagenwelt, durch den Eindruck von Kraft, Ruhe und Einsamkeit, von Weichheit und nachdenklicher Haltung angenehm berührt. Derartige Aussagen trugen zumeist den Charakter der Marbeschen Bewusstseinslagen an sich.

Von geringer Bedeutung war bei unseren Versuchen die *Deutlichkeit* des ästhetischen Gegenstandes, insofern nur *Vp B* in einigen Fällen von diesem Kriterium für ihre Beurtheilung des Eindrucks Gebrauch machte. Vielleicht dürfen wir auch diesen Gesichtspunkt dem Princip der Zusammengehörigkeit unterordnen, oder für die Erkennbarkeit des Sinnes in Anspruch nehmen. Bei allen *Vp* hat ausserdem die *technische Ausführung* gelegentlich für ihre Würdigung des vorgezeigten Gegenstandes eine Rolle gespielt. Bei *Vp A* ist bemerkenswerther Weise neben dem Urtheil : gute bzw. schlechte Arbeit oder Ausführung, auch das quantitative Moment in Betracht gezogen. Dass ein Werk anscheinend viel Arbeit gemacht hat, ist für sie auch eine Empfehlung desselben. Alle derartigen, die Technik betreffenden Aussagen sind theils dem Princip der Zusammengehörigkeit, theils dem Fechner'schen Associationsprincip zu subsumieren.

Schliesslich sind bei allen *Vp Organempfindungen* im Sinne eines von Groos näher geschilderten intendirten Nachahmung einer gesehenen Haltung oder einer vorgestellten Bewegung vorhanden gewesen. Sie sind nur bei Bildern menschlicher Körper hervorgetreten, und auch hier nur wenn die wahrge-

nommene Stellung eine besonders lebhaft oder ungewöhnliche war. Eine allgemeine Bedeutung kam ihnen nicht zu, sie dienten nur dem intimeren Verständniss einzelner Stellungen. Dadurch wird bestätigt, was ich in meiner Kritik von Groos über seine Theorie der inneren Nachahmung gesagt habe.¹ Von einer einfachen Einfühlung gegenüber Säulen, wie sie Lipps als typisch beschrieben hat, ist bei unseren Beobachtungen trotz mehrfacher darauf gerichteter Fragen an die *Vp* nichts zu bemerken gewesen. Es scheint mir das nicht unwesentlich für die Beurtheilung der ästhetischen Mechanik von Lipps zu sein.

So viel über den Ertrag dieses ersten experimentellen Versuches auf ästhetischem Gebiet mit Hilfe begrenzter Expositionsauer von Objekten. Es eröffnet sich hier, wie es scheint, ein sehr ergiebiges Feld für weitere Untersuchung. Die Objekte lassen eine grosse Variation zu und fordern sie, und nicht minder wird es von Interesse sein, die Expositionszeiten zu verkleinern und zu vergrössern. Schon die hier an drei sehr gebildeten, und praktisch sowie theoretisch auf ästhetischem Gebiet erfahrenen *Vp* gewonnenen Resultate weisen ferner auf die Nothwendigkeit, eine möglichst genaue Analyse der Aussagen vorzunehmen. Auch die individuelle Differenz der Urtheile über denselben Gegenstand ist sehr lehrreich, und verdient einer weiteren systematischen Prüfung unterzogen zu werden. Bei der von uns gewählten Expositionszeit ist das Uebergewicht des Sinnes und Ausdrucks über den direkten Faktor bereits entschieden gewesen. Es ist zu vermuthen, worauf auch die Versuche von Calkins hinweisen,² dass Kinder und ästhetisch unerfahrene Subjekte sich unter denselben Bedingungen anders verhalten werden. Namentlich aber dürfte für das Detail der ästhetischen Erkenntniss und für die genauere Differenzirung der massgebenden ästhetischen Kriterien und Principien auf diesem Wege viel zu gewinnen sein.

¹*Op. cit.*, 915 ff.

²*Psychol. Rev.*, VII, 580 ff.

A STUDY OF THE ACCURACY OF THE PRESENT METHODS OF TESTING FATIGUE.

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A variety of methods of testing fatigue have been applied to school children in recent years, and volumes of results reported ; but there has been nowhere a proper testing of the validity of these methods as tests of fatigue. Leuba, and others, have shown that the æsthesiometer test is worthless.¹ Bolton has reported experiments which indicate that the ergograph method is not accurate.² Thorndike, in a series of tests, both mental and physical (including addition, multiplication, tests of attention, memory, etc., and the dynamometer), has failed to find fatigue indicated by these tests at times when it seemed that fatigue must certainly be present.³ But a full comparative test of all the methods, with a view of testing the validity of the methods themselves, has not been reported ; and the ergograph, the reaction time, the adding, the memorizing of nonsense syllables, the filling of blanks in printed matter, are all still used as means of testing fatigue, and the results supposed to be of value. With a view of testing these methods of testing fatigue, the following experiments were undertaken.

I.

A group of five serious, advanced students and one professor was tested twice a day for five days, using more than one method of testing at each period. The tests were all short and the labor of them was not arduous enough in itself to increase to any considerable extent the fatigue present at the beginning of the test, so that practically the same condition of

¹*Psychological Review*, VI, 573 and 599.

²*Psychological Review*, VII, 136.

³*Psychological Review*, VII, 466 and 547.

fatigue was present through all the tests made at one period. The time required for the use of four tests at one period was about twenty minutes.

If these tests are accurate measures of the fatigue present, there should be agreement in results among the several methods employed at the same time. If the different tests should fail to agree in results: for instance, if in the morning test different conditions of fatigue were indicated by the different tests used at that period, or if in the afternoon tests, one test should show increased power over the morning test, another decreased power, and a third unaltered power, then some, or possibly all, of the methods must be inaccurate.

The first tests compared were the reaction time and ergograph tests. For the reaction time, the ordinary drop shutter and Hipp chronoscope were used. The reaction time tested was for the simple recognition of familiar names, such as Gray, Hume, Pope, printed in large letters on white cards. Each name contained four letters, hence the letters occupied the same space on the card in each case.

The ergograph was after the pattern of Mosso's, with an endless tape to record the total movement of the weight, and a lever writing on a smoked kymograph drum to record the number and variety of contractions. The subjects all made uniformly rapid contractions, keeping time to the beat of a metronome. Each subject was required to crook his finger till it touched a wooden stop which was placed so that it would be touched as the finger was almost completely crooked. The amplitude of the contractions thus varied with the size of the subjects' hands, but was fairly constant for each subject. The subject was required to continue his contractions till he could no longer bring his finger up to this stop. The weight was, of course, kept constant for each subject during the entire series of experiments.

The experiments were painful, but all the subjects entered into them earnestly — certainly far more faithfully than would ordinary students when being tested for fatigue.

After a short preliminary practice to familiarize the subjects with the apparatus and the tests, a series of five tests on alternate days was made; the first test between eight and nine in

the morning, before any class work was done, and the second between twelve and one o'clock after a morning of work.

At each test the reaction time was taken first. Seven reactions, exclusive of "false alarms," were made at each sitting. The last five only were counted each time in computing the average time and the mean variation. The outside conditions were kept as favorable and as uniform as possible.

It was supposed that increased reaction time and increased variation would go together and would accompany increased fatigue and decreased power as shown by the ergograph. The experiments resulted otherwise. Of 27 perfect tests only 13 times did average reaction time and mean variation of reaction time increase or decrease together; only 16 times did the mean variation in reaction time and power, as shown by the ergograph, agree in results (*i. e.*, wide variation accompany small power, or *vice versa*); only 10 times did shortened reaction time accompany increased power as shown by the ergograph, or *vice versa*. This comparison seems to show that one or both of these tests must be inaccurate.¹

Taking separately the results from each form of test, it was found that out of 27 perfect tests, 14 times was the mean variation for reaction time less, 12 times the reaction time shorter, 13 times the power as shown by the ergograph greater at the end than at the beginning of the morning. Only 6 times did less variation, shorter reaction time, and greater power with the ergograph, or their opposite, all occur at the same test together. The tests seemed to show that the students were as fresh at noon as at the beginning of the day. The appearance and feeling of the students belied the results.

In order to try the tests with yet more severe fatigue, the same group was tested one day at 8 A. M., and again at 5 P. M., after unremitting labor except during the dinner hour. Of these 6 tests, 3 times the variation in reaction time was less, 3 times the ergograph showed more power, and 3 times the re-

¹Complete tabular statements of the detailed results in this and other cases accompanied the manuscript of this article as originally submitted by the authors. The negative thesis which the article supports seems, however, so amply demonstrated by the summaries in the text that, on the recommendation of the editors, the full tables have not been inserted.

action time was shorter in the afternoon than in the morning ; 3 times the three tests agreed in results.

II.

These methods seemed so unreliable as tests of fatigue that it was decided to add others with less motor and more intellectual elements. In the new series the ergograph was dropped, as it seemed no test of fatigue and was troublesome to use. The same subjects were used as before. Four forms of test were used : (1) reaction time (taken as before), (2) addition of columns of figures, (3) writing the cubes of numbers up to 9, (4) memorizing nonsense syllables.

The figures were printed in columns of three figures, each 27 figures deep, thus making it a task of adding units, tens, and hundreds. Both the total number of figures added and the number of columns added correctly were counted. In order that the addition of each column might be considered separately, as well as the sum as a whole, the sum of each column was written beneath it, and not just the right hand figure as is customary. In this way the amount "carried" each time was indicated, and a mistake made in one column, which caused an incorrect result in the next column, even though that was added correctly, could be discovered.

The figures to be cubed were printed in vertical lines, and the subject wrote the cube in a blank space at the right of each figure, all multiplication being mental. After a few tests it was a mere matter of memory and manual dexterity. Both the number of cubes written at a test and the accuracy of results were considered.

The nonsense syllables, containing three letters each, were printed in horizontal lines, ten in a line. The subject was required to memorize these in the order printed, and at the end of the period to turn the paper over and write, in correct order, the syllables remembered, and to state what number he had attempted to memorize. In tabulating these results the following points were considered : (1) the number attempted, (2) the number learned correctly, (3) the number incorrectly recalled, (4) the number omitted, (5) the number transposed.

In taking the last three tests, all were seated around a large

table with everything in readiness. All began and stopped at a given signal. Two minutes were allowed for adding, when papers were changed and two minutes given for cubing. After a change of papers two minutes were given for learning nonsense syllables, and finally, two minutes allowed for writing down the syllables remembered. Tests were taken at 8.30 A. M., 12.30 and 5.30 P. M., each day for four days. The time chosen was examination week, when all students were working very hard and were under fatiguing mental worry.

The results of these experiments were equally disappointing. Comparing the 8.30 A. M. and 5.30 P. M. tests, we found the following results: Of 24 perfect records for each test, the reaction time and variation increased or decreased together 13 times; the reaction time test agreed with the addition test 13 times (*i. e.*, short reaction time accompanied a large number of figures added, or *vice versa*), with the cubing test 10 times, with the memorizing-nonsense-syllables test 6 times; the mean variation in reaction time agreed with the adding test 11 times, with the cubing test 10 times, and with the nonsense test 8 times; the adding and the nonsense tests agreed 8 times; the adding and cubing tests agreed 10 times; the cubing and the nonsense tests agreed 11 times.

Comparing now not the total work done, but the total done correctly at 8.30 A. M. and 5.30 P. M., we found: Of 24 perfect tests, the reaction time test agreed with the addition test 15 times, with the cubing test 11 times, with the nonsense syllables test 5 times. (This estimation of accuracy of the work in learning the nonsense syllables is of questionable accuracy because of the difficulty in properly estimating transpositions, etc.); mean variation agreed with addition 11 times, cubing 10 times, and nonsense syllables learned 8 times; addition agreed with cubing 12 times, and 4 times with nonsense syllables; cubing and nonsense agreed 11 times. On no day did all the tests agree in results, though on one day with one person all tests but the variation in reaction time agreed in showing better condition at 5.30 than at 8.30, and on another day, with one person, all tests except the nonsense syllables agreed again in showing better condition late in the afternoon.

Taking each form of test separately and comparing 8.30 A. M.

and 5.30 P. M. tests, we found : Of 24 tests, reaction time was longer in the afternoon only 10 times, variation greater only 11 times, total number of figures added less only 7 times, number added correctly less only 9 times, number of cubes written less 7 times, number of cubes written correctly less 5 times, number of nonsense syllables learned less 9 times.

This would seem to indicate a better condition at 5.30 P. M., after a day of hard study and worry, with one or two three-hour examinations often added, than at 8.30 A. M., after breakfast and a short walk. In some cases there could be no doubt that the subjects were very severely fatigued in the afternoon, though it was not often shown by the tests. One of the best records made by one of the students came just after a long, hard examination for which the student, after working and worrying all the day before, had continued work till two o'clock the night before the test.

III.

With the purpose of testing the matter with other subjects and of introducing another method, which offered more interesting matter to work with, the experiments were repeated a year later with 7 subjects, during the week before the March examinations, the week of the examinations, and the week afterwards — this last week came after three consecutive days of rest from all school work.

The tests employed were exactly the same as the year before with the addition of the combination method of Ebbinghaus, *i. e.*, filling in blanks which were made by omitting letters and small words from printed matter. The material for this test was made as follows : selections from Hamerton's *Essays on Human Intercourse* were printed with letters left out at intervals, sometimes one, sometimes enough to make a whole word. A short dash indicated the position of each missing letter. Earnest effort was made so to cut out these letters as to make it as difficult to fill the blanks in one group of lines as in another. In this we failed miserably, though half a dozen different pieces of literature were worked with before we settled on the monotonous Hamerton. It is simply impossible to make material of even approximately uniform difficulty.

If the test lasted half an hour or more, the difficulties of the lines used at each period would probably sum up about the same, but in short tests this is far from the case.

A comparison of the results of the tests taken at 8.30 A. M. and 5 P. M. during the first week, gives practically the same results as those just recorded for the previous year. The agreements and disagreements between the results of the different tests were sometimes more, sometimes less, never much over fifty per cent., often much below it. The only result which seemed even partially favorable was the learning of less nonsense syllables in the afternoon 10 out of 14 times.

Considering the results of the new test of filling in the blanks in the reading matter, we found that out of 14 perfect records more blanks were filled in the afternoon 9 times ; this test agreed in results with the reaction time test 9 times, variation in reaction time 7 times, adding test 3 times, cubing test 6 times, nonsense syllables test 7 times, when total amount of work was considered. When only the accurate work was considered in each case, the results were equally hopeless. In no instance did all the tests agree.

During the next week a change in the method of taking the reaction time was adopted, so that a choice of two reactions was made necessary instead of the simple reaction. This was done because of the utter lack of reliability in the simple reactions. Five reactions were taken at each sitting as before.

Of 24 perfect tests during this week the same lack of agreement between tests, and the same failure to indicate fatigue in the afternoon were found as before. Reaction time and variation did agree 16 times, and variation and cubing agreed 16 times, but reaction time was shorter in the afternoon than in the morning 16 times. Only 10 times out of 24 was the number of nonsense syllables learned less in the afternoon during this week, as against 10 out of 14 times the week before ; while 15 out of 24 times there were fewer blanks filled in the afternoon this week against 5 out of 14 the week before. The other results were utterly irreconcilable. Never did all tests agree.

The third week's test showed practically the same results. The reaction time was greater in the afternoon 18 out of 25

times this week, and agreed with the variation 15 out of 25 times; the number of nonsense syllables learned was greater in the afternoon 11 times out of 25. The number of blanks filled was greater in the afternoon 16 times — almost the exact opposite of the results of the previous week.

Thinking that 8.30 A. M might be too early, and the students not yet "warmed up" for the day, three subjects were now tested between 9 and 10 A. M. and 5 and 6 P. M. each day; one subject for four days, one for three, and one for two. The same tests were used, and results equally inconsistent with each other and with the undoubted facts were obtained.

It was perfectly plain that when used under exceedingly favorable conditions, by careful experimenters and with intelligent and earnest subjects, each and all of these tests were utterly unreliable as measures of fatigue in cultivated adults.

IV.

With a view of determining whether they could measure the condition of untrained children better, a new set of experiments was tried with sixth grade pupils as subjects. It was thought that possibly the habit of forced concentration and the cultivated will of a college student, would enable him to work well for the short period of the tests in spite of fatigue, while the younger pupils might not be able thus to hide their condition.

Ten children of average ability, ranging from 11 to 15 years of age, were tested for three days at 9 A. M. and between 2.45 and 3 P. M., the opening and closing hours of their school. A vacant room in the school building was used. The experiments were made by us but with the co-operation and entire sympathy of the principal of the school. The pupils entered into the tests eagerly and with remarkable earnestness. The conditions seemed ideal. The adding, cubing, learning nonsense syllables, and filling blanks were the tests used. Special pieces of easy reading were prepared for them, and the story contained in the printed matter read to them each time just before the test was made, thus making it a test of memory as well as of other mental powers.

Out of all these tests, once only, and with one boy, did all

the tests show the same thing—in this case they showed greater power at 2.45 P. M. than at 9 A. M. The failure of agreement in the tests was about the same as with adults. For instance, the addition test agreed with the cubing test 13 out of 27 times, with the filling blanks test 14 out of 29 times; the cubing test agreed with the nonsense syllables test 15 out of 27 times, and with the filling blanks test 14 out of 26 times. The accuracy in addition and in filling blanks agreed 12 out of 29 times.

The failure to indicate fatigue in the afternoon was even more pronounced than with adults. Of 30 tests, less figures were added in the afternoon only 9 times, less columns were added correctly only 10 times. Less cubes were written only 3 out of 27 times; the number of nonsense syllables correctly remembered was less only 10 out of 30 times; and the number of blanks filled was less only 9 out of 29 times.

V.

A further test has this year been made with the ergograph, repeating a part of the work done by Keller.¹ Keller tested a school boy with the ergograph at frequent intervals during several days, giving him mental work between the tests. The mental work consisted in pronouncing lists of German words, and some rows of figures, prepared for this purpose. The records of two days tests are as follows :

FIRST DAY.

8	A. M.	Ergograph showed power equal to	.9776 Kgm.	
		Read 1,386 words, average time per word		.3515 sec.
8.35		Ergograph showed power equal to	1.491 Kgm.	
		Short pause. Read 1,257 words, average time per word,		.338 sec.
8.50		Ergograph showed power equal to	1.8632 Kgm.	
		Short pause. Read 425 words and some figures, average time per word		.354 sec.
9.05		Ergograph showed power equal to	1.299 Kgm.	
		Pause. Subject walking.		
10.15		Ergograph showed power equal to	.817 Kgm.	
		Read 1,364 words, average time per word		.358 sec.

¹*Biologisches Centralblatt*, XIV, Nos. 1, 2, 9.

10.35	Ergograph showed power equal to Pause. Read 399 words and some figures, average time per word	1.857 Kgm.	.349 sec.
10.50	Ergograph showed power equal to Short pause. Read 1,217 words, average time per word,	1.6962 Kgm.	.326 sec.
11.03	Ergograph showed power equal to Pause.	.864 Kgm.	
12.15 P. M.	Ergograph showed power equal to Pause.	.8298 Kgm.	
3.20	Ergograph showed power equal to Read 1,396 words, average time per word	1.0466 Kgm.	.305 sec.
3.47	Ergograph showed power equal to Read 1,266 words, average time per word	1.63 Kgm.	.283 sec.
4.05	Ergograph showed power equal to Ergograph showed power equal to Pause.	2.156 Kgm. 1.088 Kgm.	
5.20	Ergograph showed power equal to	.8657 Kgm.	

SECOND DAY.

8.10 A. M.	Ergograph showed power equal to Read 1,392 words in 7.5 minutes, average time per word	1.4294 Kgm.	.328 sec.
8.30	Ergograph showed power equal to Short pause. Read 1,270 words in 7 minutes, average time per word	1.7206 Kgm.	.324 sec.
8.45	Ergograph showed power equal to Read 1,267 words and some figures in 8 minutes, average time per word	2.2568 Kgm.	.324 sec.
9.00	Ergograph showed power equal to Pause.	1.2436 Kgm.	
10.15	Ergograph showed power equal to Read 2,457 words in 14.5 minutes, average time per word	.7888 Kgm.	.346 sec.
10.30	Ergograph showed power equal to Pause 10 minutes. Read 2,501 words in 14-15 minutes, average time per word	1.2752 Kgm.	.356 sec.
11.00	Ergograph showed power equal to Pause.	.785 Kgm.	
12.00 M.	Ergograph showed power equal to	.3838 Kgm.	

The blind infatuation for this ergograph test is clearly shown here. It would seem much more reasonable to think that

one's ability to recognize and to call out words would come nearer indicating the condition of his mind than would one's ability to crook automatically one finger; yet Keller ignores the evidence of the time required to pronounce the words, and draws his conclusions entirely from the ergograph records.

He decides, for instance, that there must be great exhaustion during the periods from 10.50 to 11.03 A. M., and 12.15 P. M., on the first day, because the power, as shown by the ergograph, fell from 1.692 Kgm. to .864 Kgm. in 13 minutes; yet the average time required for recognizing and calling a word was not so great between 10.50 and 11.03, as between 10.35 and 10.50. If either of these methods is an accurate test of fatigue, then children are in better condition in the afternoon than in the morning, for at every test during the afternoon the words were called in less time than at any morning test, and the greatest power shown by the ergograph was at about 4 P. M.

Such unguarded experimentation really shows nothing. Crooking a finger to exhaustion every 15 minutes is a painful procedure, and no average boy will do it with uniform effort, whether fresh or fatigued. The fact that twice the ergograph results vary practically fifty per cent. in consecutive tests only 13 minutes apart, should suggest the unreliability of such a test as a measure of mental fatigue. The effects of practice, of local soreness and local fatigue, and of the changing mental attitude of the subject were not considered as they should have been.

In order to find whether the mental work had anything to do with the irregularities of this irregular ergograph record during the day, a boy 14 years old (the same age as Keller's subject), was tested by us for two days and was paid to sit around the laboratory and do nothing between times. He was naturally lazy and used to the house. The laboratory was large and comfortable, and he was free to walk around and lounge at his pleasure. He knew us well and was quite at his ease; he did as near nothing, mentally and physically, as one well could. The record is as follows :

FIRST DAY (MIDDLE FINGER, RIGHT HAND).

8.15 A. M. Ergograph test, weight lifted 78 inches.

8.50	"	"	"	"	107	"
9.05	"	"	"	"	74	"

Short walk.

10.35	Ergograph test, weight lifted 140 inches.				
10.55	"	"	"	"	89 "
11.10	"	"	"	"	80 "
11.23	"	"	"	"	102 "
12.30 P. M.	"	"	"	"	153 "

SECOND DAY (FOREFINGER).

8.35 A. M.	Ergograph test, weight lifted 64 inches.				
8.55	"	"	"	"	70 "
9.10	"	"	"	"	59 "
9.25	"	"	"	"	53 "

Walked several blocks.

10.40	Ergograph test, weight lifted 66 inches.				
10.55	"	"	"	"	48 "
11.10	"	"	"	"	40 "
11.25	"	"	"	"	46 "

Walked several blocks.

12.25 P. M.	Ergograph test, weight lifted 65 inches.				
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The weight used was five pounds. The middle finger of the right hand was used the first day, and the forefinger the second day. The change was made because of the difficulty in properly adjusting the hand for the use of the middle finger.

The most noticeable things in these records are : 1st. The power, as indicated by the distance through which the weight was lifted, varied all the way from 74 inches to 153 inches on the first day, and from 46 inches to 70 inches on the second day; 2nd. The tests after the hour intermission and walk, and the last test before dinner on each day, showed very great power. The variations were wide and irregular, without any work being done on either day.

On another day four boys were tested by the method employed by Smedley, in Chicago.¹ Two were required to work arithmetic examples between the tests, while two did nothing but walk around the building and lounge in the laboratory. Both of the boys who did not work showed a gain of power for the first three tests, but both showed a falling off at 12.20, the last test of the morning, and both showed slightly less power than at 9 o'clock. One of the boys, who worked all the

¹Report on Child Study Investigation, Annual Report Board of Education of Chicago, 1898-'99.

time, improved regularly until 11.30, and showed only a slight falling off at 12.20, although his record at that time was higher than at either of the first two tests of the morning. The record of the other boy who worked was marred by poor adjustment of the hand in the last test ; but at the 11.30 test he was just as strong as at the first test. All of them showed a falling off in the afternoon except one working boy who showed a slight gain of power the first test after dinner, but a marked decrease in the later tests. The other three boys did not show very much loss of power in any of the afternoon tests. They were all quite tired of (or bored with) the experiment by this time and were anxious to get out. They really did not seem to try so hard after dinner as in the morning.

VI.

After three years of periodic trial of all these methods of testing fatigue, the results force upon us the conclusion that they are, as used at present, worthless as tests of fatigue either in children or in adults. Two things further seem to us quite clear : 1st, That no such test extending over so brief a period as two to five minutes can be accurate in measuring fatigue, because any average student can, by a strong effort of the will, practically nullify for such a period the effect of any fatigue that is not pathological ; 2nd, On the other hand, the tests continuing for an hour or more, using uniform work, such as addition, are of necessity made with material that is not uniform, or is uniformly stupid and uninteresting. No interesting work seems to be measurably uniform in difficulty or uniformly interesting.

The problems of fatigue will never be settled till such tests are abandoned and the results gotten by them recognized as worthless. When we surrender our present vain knowledge, and come to the problem fresh and unbiased, there is hope of solving it.

The first thing needed is a clearer analysis of the conditions present when one is fatigued ; now, very varied states are roughly classed as states of fatigue. When the components of fatigued states are better known, then a method must be devised, either to eliminate in turn each component or to measure

its effect. Thorndike is the only other worker in the field who has properly recognized the complexity of the fatigued states and attempted to analyze them. He, unfortunately, did not follow up this line and try to eliminate or to measure and evaluate these several components. A part of this work we have roughly planned but have yet to complete.

Before any specific state of so-called fatigue can be intelligently considered, several facts must be determined.

1st. What part of the apparent fatigue (or inefficiency), is due to purely local physiological conditions?

2nd. What part to purely mental conditions?

(a) What part is due to suggestion, conscious or unconscious?

(b) What part is merely lack of interest, or "bore?"

(c) What part is due to a pure mental fatigue?

3rd. How far can the will of the person tested go in hiding or overcoming the effects of each of the elements of fatigue, and how long can this effort be maintained?

4th. Does the test employed extend beyond this period of volitionally modified effort; and, if so, does it not itself produce the fatigue it may measure?

5th. What is the emotional attitude of the subject toward the test employed, and how far will this influence the results in each case even with conscientious subjects? Can any test be found of uniform interest to any large number of pupils?

When it is recognized that "fatigue" is itself not the simple matter it has been considered, and that the attitude of the subject towards the test largely determines the result, it will be readily recognized that the search for a single, simple, accurate test of fatigue that can be employed at any time with any number of pupils by a half-trained teacher is a vain quest.

A NEW TYPE OF ERGOGRAPH, WITH A DISCUSSION OF ERGOGRAPHIC EXPERIMENTATION.

By Professor JOHN A. BERGSTRÖM, Indiana University.

Apparatus for the study of muscular strength and endurance may be arranged in a series beginning with that designed like the common myograph or Mosso ergograph for the study of the action of isolated muscles or single muscle groups and ending with that which brings into play many, or as nearly as may be, all the muscles of the body.

Excellent recent examples of the latter type may be found in the physiological laboratory of the Carolinska Institute, Stockholm, and in that of the University of Lund. The apparatus at the Carolinska Institute is made on the plan of a weight rowing machine, and is equipped with elaborate devices for regulating the rate and character of the work, and for registration. Together with the man subject to experiment, it is enclosed in a large air-tight chamber, the gaseous contents of which may, from time to time, be chemically determined. It seems admirably arranged for the study of many of the conditions, and also effects, of muscular work of a complexity and difficulty comparable with that of many forms of ordinary labor. The apparatus, planned by Professor Blix of Lund, resembles a bicycle, the resistance to motion being furnished by friction applied to the axis of what corresponds to the driving wheel. An ingenious recording mechanism gives a continuous curve of the pressure exerted throughout the work. This piece of apparatus is intended especially for the study of the greatest possible muscular effort and seems well adapted for its purpose.

Most forms of hand dynamometers and dynamographs may be said to occupy an intermediate position. One of the simplest and best of recent models can be seen in the laboratory of Dr. Alfred Lehmann, at the University of Copenhagen. It consists essentially of an adjustable spring dynamograph attached to a base, which also carries a transverse ridge, against

which the thumb and back part of the hand are braced in making records. The position of the hand seems firm and convenient.

In the description of the ergograph in *Du Bois-Reymond's Archiv*, 1890, Mosso points out very clearly the advantages, for the analysis of neuro-muscular activity and fatigue, of the isolation of the muscle to be used, and of making the conditions of load and registration as nearly as possible like those in experiments with excised muscles.

This ideal he does not, however, realize in his own work, as he recommends the use of the whole middle finger in experiments, the collar for the attachment of the weight being placed around the second phalanx. Several muscles are involved in the movement, the lumbrical and palmar muscles as well as the flexor sublimis and flexor profundus, which he appears erroneously to have believed to be the only ones concerned. To secure a better isolation of the action of these two muscles, Kraepelin recommended the fixation of the first phalanx and the movement of only the last two; this suggestion appears to have been adopted in most, if not all, of the more recent types of apparatus.

The nearest approach to complete isolation has thus far probably been obtained in experiments with the abductor indicis first recommended for this purpose by Fick¹ and recently adopted by Lombard in a late type of ergograph.

I.

With the instrument to be described in this paper, it is possible to obtain practically the complete isolation of the flexor profundus in its application to each finger of either hand; also, almost, if not quite, the complete isolation of the abductor indicis and the abductor minimi digiti. In addition experiments may be made in which the flexor sublimis, while not isolated, plays much the leading part. Records may also be obtained from the flexion of the second and third phalanges together, from the extension of the third, or second and third phalanges, and from the flexors and extensors of the thumb. However, experiments

¹A. Fick: *Myographische Versuche am lebenden Menschen. Pflüger's Arch.*, Vol. XLI, 1887.

with the flexores sublimis are likely to be rare, inasmuch as experiments with the flexors of the third phalanges and the four abductors are relatively free from objection and do not require so much exertion.¹

A slight difficulty is experienced with the abductor indicis because of its attachment to the metacarpals of the thumb as well as the first finger. At first some subjects appear to need a brace for the thumb, but this makes it easy to displace the hand. A little practice in voluntarily holding the thumb back in a certain position seems to me to remove the trouble sufficiently.

An important source of error in securing proper isolation, and particularly in maintaining the same position and condition of the moving parts of the hand, has been the fact that all types of ergographs, as far as known to me, have been placed upon rigid supports and usually clamped solidly to them. Movements of the entire arm, and even of the body, can thus be brought to bear to displace the hand, and, perhaps, to some extent, at times, to assist in raising the weight or extending the spring. With heavy loads or in nearing exhaustion in endurance tests, concomitant movements of nearly all parts of the body are to be observed.² With careful training they may in large part be suppressed, and they are doubtless much the most troublesome in beginners. This difficulty, I believe, is nearly, if not wholly, obviated by suspending the ergograph by a spring or by a cord with a counterpoise, so that it may move freely with the arm as it is pulled about in the struggle to make a maximum record. The instrument should be as light as possible to avoid the effects of inertia, and should, of course, be attached to the arm at essential points only. The

¹This instrument in a somewhat less complete form was exhibited at the Baltimore meeting of the American Psychological Association in 1900. *Psych. Rev.*, Vol. VIII, p. 168. It has been developed largely in connection with certain ergographic studies that have been in progress in the psychological laboratory for several years, and I have received assistance in many ways not only from those whose records appear in this article but from others who have co-operated in these studies, among whom I wish especially to mention Jas. W. Westfall, Clark Wissler, H. H. Niekamp, and James P. Porter.

²Clark Wissler: Diffusion of the Motor Impulse, *Psych. Rev.*, VII, 1900, 29-38.

one to be described is attached only to the hand, or in some cases to the hand and forearm, and swings freely from a spring. The fact that the arm and body movements meet only a slight resistance, and produce no alterations in the working parts, appears by itself to check their extent.

A more serious source of error with the present instrument than the above is the pressure that may be exerted by the thumb and fingers against the binding; the movements that may be made at the wrist are also troublesome. However, with care in both binding and clamping, it is possible to make these sources of error combined of so little effect that the center of rotation of the phalanx used will usually coincide quite exactly with the center gauge at the end of a fatigue record.

II.

The relative effect of the different muscles in producing movement and pressure by the fingers has been a matter of doubt and dispute. The opinion that the flexors of the second and third phalanges were also chiefly concerned in the flexion of the first would appear to have been general till the report of the observations of Duchenne, in 1885.¹ According to him the first phalanx may still be bent vigorously when the flexors of the second and third phalanges are paralyzed through disease, thus showing that the flexor sublimis and flexor profundus are not alone or chiefly concerned in the flexion of the first phalanx. Moreover, he observed that with paralysis of the extensor communis, it was possible for patients still to extend the last two phalanges. The inference is therefore made that the lumbrical and interosseus muscles in the hand are the ones chiefly operative in the flexion of the first and the extension of the last two phalanges.

An experiment like the following, while not complete, can aid in some respects, in a rough analysis. A collar with a depression to allow free play to the tendons is placed around the first phalanx, and then attached to a dynamograph; the second and third phalanges remain free. It is found that some pressure may be exerted while the last two phalanges

¹ R. Müller: Ueber Mosso's Ergographen. *Wundt's Phil. Studien*, 1901, XVII, 1-29.

remain comparatively limp, with a stronger pressure the second becomes rigid, then the third; but even with the maximum pressure it is possible for the subject to move the last two phalanges forward and backward, with respect to the first phalanx, without causing a variation in the total pressure of more than one sixth. If the last two phalanges were themselves pressing against a spring, such a movement might be due to variations in the pressure exerted against it, but this is not the case. If the flexor sublimis and profundus and the extensor communis were the only muscles concerned, these movements could not take place without reducing almost completely the total pressure at each extension of the last two phalanges, providing the flexor and extensor tendons are approximately the same distance from the center of rotation of the first phalanx. These movements could take place either if the effect of the flexors of the last two phalanges upon the first phalanx were slight, so that counteracting their force would produce only a small change, or if the tendon of the extensor concerned were much nearer the center of rotation or actually a flexor of the first phalanx. Both of these conditions are possible in this case. On account of the shortening of the flexor profundus and flexor sublimis, through the bending of the phalanges, their tension is not over a half of the maximum, and the lumbrical and palmar muscles can serve the double function of flexors of the first, and extensors of the last two phalanges.

Figure 1 gives a single record of such an experiment; the curve at the left is proportional to the total pressure of the first phalanx with the aid of all the flexors, under the conditions given; the curve at the right exhibits a similar record, but in this the flexion of the first was accompanied by simultaneous forward and backward movements of the last two phalanges. These show themselves as small variations at the bottom.

A similar but smaller record is obtained if the collar is placed about the second phalanx, and the forward and backward movements are made by the third phalanx, which is left free. Here the only interpretation possible would seem to be that under these conditions the flexor profundus contributes only a little to the force of flexion of the second phalanx.



FIGURE 1. (W. A. C.)



FIGURE 2.



FIGURE 3.



This matter will be made more definite by further experiments.

By a similar method we may attempt to ascertain to what extent the tendon and division of the flexor profundus for a given finger can act independently of the rest.

If an oval dynamometer be held in the hand so that pressure may be exerted upon it only through the last phalanges, we can obtain an indication of the maximum pressure from all simultaneously and from each separately. In a few experiments with two subjects, the sum of the separate pressures from the phalanges approximately equals the simultaneous pressure of all, being in one case somewhat less, in the other somewhat more. This suggests that only a certain definite division of the tendon and muscle are effective in the flexion of any special one of the third phalanges. The same conclusion may also be drawn from the following experiment. If the second phalanx of a given finger be clamped, while the third is free to move but is restrained a little by a small load of about a twentieth of its total lifting power to check slight involuntary movements, it will be found possible, with a little practice, to flex the free phalanges of all the rest of the fingers very nearly as much whether one simultaneously bends the third phalanx of the clamped finger or not; but it is not done with quite the same ease, and the difficulty of flexing other fingers than the one clamped varies somewhat with the different fingers.

However, a possible interpretation of a third experiment would give a contrary view of the matter. If we ascertain what maximum pressure can be exerted by the third phalanx of the clamped finger (1) when the hand is left free, (2) when the first and second, and third phalanges of the other fingers are tied down, we find that the pressure is about the same when the first phalanges are fixed as when the hand is free, but that binding the second phalanges reduces the pressure by about a fifth, and binding the third reduces it to about two-fifths.

Were it not for the two previous experiments, the simplest explanation would be that the parts of the flexor profundus and tendons adjacent to the working division were prevented from rendering the usual assistance by stretching and fixation. It

may be due partly to this, and partly to an interference with the voluntary control by the unusual conditions, and the reduction in the force might be modified by practice. Whatever it may be due to, the fact should be taken into account in binding the hand and fingers in ergographic experimentation.

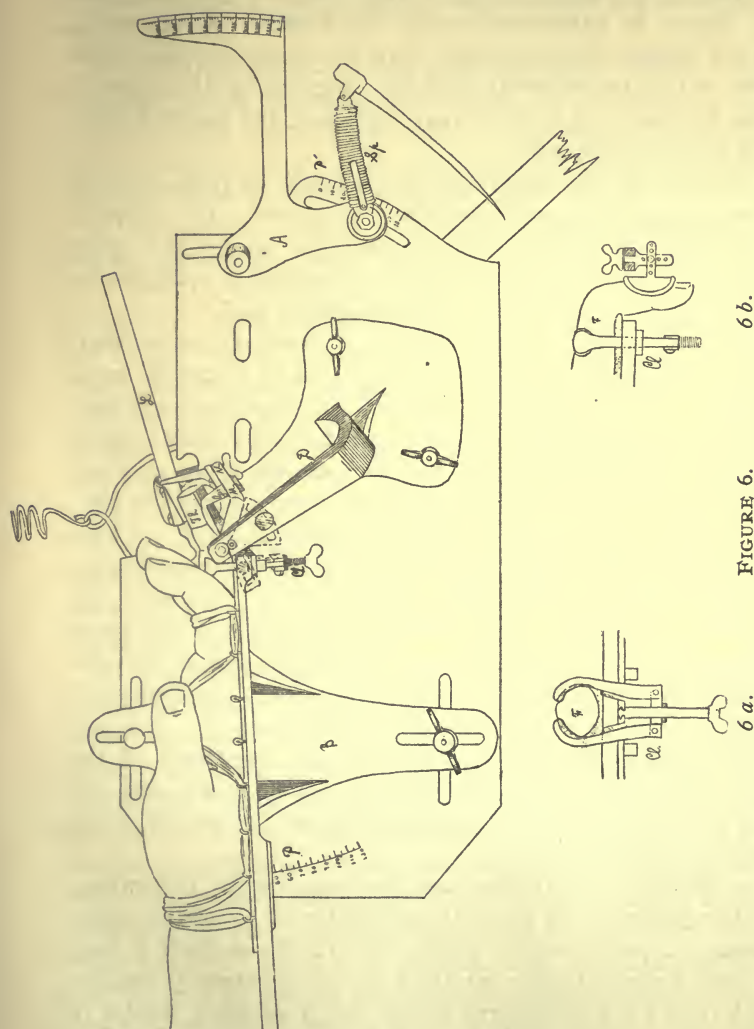
III.

In the mode of ergographic experimentation to be described, the idea in the fixation of the fingers is to find the essential center of rotation of the phalanx to be used and make it coincide with the center of rotation of the lever against which the work is done. In this case, if the phalanx is rigid, the exact place of application of the load is a matter of indifference, as the load and force of the muscle will vary in case of a change of place in exactly the same proportion, and the load will always remain perpendicular to the moving parts.

The center of rotation is found by reference to the folds of the skin and by observing the phalanx as it moves. The relations of the parts and the location of the centers of rotation will be seen in the x-ray photographs given above in Figures 2, 3, 4, and 5. The centers of the crosses are the true centers of rotation. Small pieces of lead were placed upon the spots usually selected as centers of rotation in experiments and appear as round dots. They were liable to some displacement from projection, but in three out of the five cases the crosses and dots come fairly close together. The circles were drawn to indicate approximately the circumference of the surfaces of rotation.

Figure 2 represents the bones and joints of the middle finger of the right hand; Figure 3 shows the third phalanx partly flexed; Figures 4 and 5 show the first phalanx of the first finger before and after abduction, respectively.

The manner in which the hand is placed in the apparatus for experiments with the flexors and extensors is exhibited in Figure 6. The bracket B is adjustable so that the center of rotation of the phalanx may be brought into coincidence with the center of rotation of the lever of the apparatus as indicated by the center gauge Ga.



6 b.

FIGURE 6.

6 a.

In the reproduction of the original drawing of this instrument the letters Ga have become nearly indistinguishable. They lie about the middle of a straight line drawn from the letter D to the tip of the middle finger.

In experiments with the extensors the hand is placed palm down and the adjustment and application of the load is made as shown by 6 b, while Figure 6 itself exhibits the same facts with regard to experiments with the flexors. Experiments with the flexors of the thumbs may be made with the same means, but for experiments with the extensors of the thumb a special bracket, which would permit placing the fingers below, would have to replace B.

A device of special importance is the clamp C1 (Fig. 6 a) for holding the phalanx preceding the one moved. If a circular band is used, it may be made so tight, even within the limits of endurable pain, as to cause a very considerable reduction in the extent of movement and in the maximum pull.

This is probably due to the friction produced on the tendons. If, on the other hand, the band is loose, so as to permit some movement, both the height of lift and the maximum pull will be considerably increased, and we shall have the effects not of an isolated muscle but of the partial co-operation of two or more. In any case the use of the band or plain circle about the phalanx produces variations not easily taken into account. These sources of error may, I believe, be wholly avoided in experiments by careful use of a clamp like C1 (Fig. 6 a). It leaves the space over the tendons free; moreover, the width of this space varies automatically with the size of the finger, and as the jaws are forced down towards the bone they at the same time push the flesh up into a ridge, thus preventing binding. It is adjustable to fingers of any size simply by the movement of the screw. The clamp moves in a slot near the edge of B, and can be set at any point, as can be also the adjustable holder of the thimble, attached to M M.

The phalanx in the clamp must be fixed solidly, if accuracy is to be attained. To make it sufficiently so, it has not been necessary to pass the pain threshold with the subjects I have had, and a great enough variation in the tightness is permissible to make the adjustment of the clamp merely a matter of ordinary care. This applies to all adjustments except that for movements of extension, in which case felt must be put under the jaws, as the pressure becomes speedily painful. One may

judge that the clamp is sufficiently tight either by the fact that the phalanx clearly cannot be moved, or by the fact that any further increase in the pressure does not reduce the heights of the lifts.

To secure accuracy two other matters must receive careful attention. A is an adjustable arrest for the lever L. The angle at which the phalanx begins its movement is determined by the relative position of A and B, which may be made definite by reference to the circular scales P and P'.

If the purpose is simply to secure the same position of the phalanx at different times, it will be sufficient to move A and B to the same readings and center the phalanx by aid of the gauge, Ga. If, however, we wish to make comparative records, and to have the angle the same under these different conditions, the distance of the center of rotation above the top of B must be taken into account in adjusting by scale P. The importance of this angular adjustment will be evident from Figures 7 to 10 below. The maximum pull diminishes regularly to about one-half with a change usually of less than 50° and the angular movement of a muscle loaded with one-half or more of its maximum is nearly to the same point, whatever the place of starting may be. An angle of 180° may be taken as the standard starting point.

This angular error is also the chief one to be considered in the adjustment of thimble, Th. It may be placed nearer or farther from the center of rotation without affecting the record much, but a variation in the tightness with which it is forced on the finger leads to considerable differences because it makes the angle at which the phalanx starts greater or less. In practice the thimble is forced onto the finger till it is even with the end. A corresponding degree of tightness is necessary for comparative records and in any given case the number of the thimble used should be noted. A thimble with a top adjustable by screws might for certain purposes have some advantage, but the plan adopted was to have a set of thimbles at hand from which one of the proper size is selected.

The second of the two precautions referred to above is proper centering. Both the angle of starting and the distance of the load from the center of rotation will vary in ways that will be easily imagined from the geometrical relations of the parts in-

volved. Placing the center of rotation of the phalanx ahead of the center gauge, for example, will give it an advantage at the beginning of flexion but a relative disadvantage at the end, both because the lever of application of power is longer than at the start and because the angle of movement is relatively greater than it would be if the center coincided with the edge of the center gauge.

If an instrument of measurement is to be completely adapted to its purpose, it must give its results in absolute units. For some ergographic problems, however, it would suffice if it were made possible to use a muscle or group of muscles again under exactly the same instrumental conditions, so as to give a series of comparable records with the same member. By attention to the adjustments discussed above this may easily be done with the ergograph here described.

A still higher degree of usefulness would be attained if experiments could be made on different phalanges of the same or different persons under conditions sufficiently similar to make even such records accurately comparable. This, too, is evidently possible, so far, at least, as we may assume that different phalanges and joints are similar.

It remains, therefore, to see how nearly we can refer the records to the elementary geometrical and mechanical properties of the working parts and to ascertain to what extent they may be directly compared with the records obtained from excised muscles in the myograph, with which such reference is possible.

I have not had the opportunity of attempting to solve this problem, as did Mosso, for the whole finger, by a direct comparison between the shortening of the muscle and the angular movement of the phalanx, but the facts may, I believe, be inferred from the appearance of the joints in Figures 2 to 5. The bearings of the phalanges appear to be circular. If the flexor tendon were attached directly at the surface of rotation of its phalanx and moved over the circular surface of the one with which this articulates, the amount of movement of the tendon would clearly be directly proportional to the angular displacement of the phalanx. This will also be true at whatever point the tendon may be attached to the phalanx, as long as it rests upon the circular surface of the preceding one, and it would be

approximately true for a considerable angular distance beyond that at which the tendon would naturally pull away from the surface, if it were kept near it by ligaments. This applies also to the muscle attached to a succeeding phalanx, as, for example, to the flexor profundus in the rotation of the second phalanx. Such a muscle will, of course, not be effective till the phalanx to which it is attached has reached its own limit of motion, unless it be counterbalanced by an extensor which is nearer the center of rotation or is at the same time a flexor of the preceding phalanx.

For our present purpose the important point to examine is, therefore, whether in the bending of the phalanx the point is ever reached where the tendon is lifted from the surface of rotation so as to alter the radius of application of the force. In feeling of the joints in flexion the tendon becomes a little more prominent in the second, but hardly appreciably so in the third. From a consideration of the relative position of the phalanges in the x-ray picture, it does not seem probable that with the usual degree of flexion in ergographic experiments, the tendon will change radius with respect to the rotation of the phalanx to which it is attached. That there will be a tendency to such a change in the relation of the flexor profundus to the second phalanx seems probable. This may explain the peculiarity of Figure 9 and the difference between this and Figure 10 below. The fairly regular gradations of heights in all figures except 9 is another evidence that there has been no change in the mode of applications of the force.

These conclusions may be generally valid, but it is possible to conceive of such individual differences in structure and attachment of tendons as to make them only approximately correct for some.

If we assume that they are generally valid, then we have in experiments with the flexors of the last phalanges, and with the abductors of the first and fourth fingers, nearly ideal general conditions for ergographic work, namely, practically complete isolation of the action of a single muscle, angular movements of the phalanx directly proportional to the shortening of the muscle, and a possibility of calculating the extent of movement and the actual force exerted by the muscle, approximately

without, and very closely with, the aid of x-ray photographs. If the force of contraction of the flexor of the last phalanx is known, we may calculate the strength of the flexor of the second from the force exerted by the two combined, and we may then proceed to ascertain the effect of both in the flexion of the first.

IV.

While the reference of the records to elementary geometrical properties and relations is the only foundation for obtaining absolute and generally comparable results, we can, for certain purposes, refer them to another standard, namely, to the variations in the height of the lift with a given load. The changes produced by fatigue may thus, for example, be compared with those due to increasing the load.

To ascertain approximately what the change in the height of lift is in response to a series of equal additions to, or subtractions from, the load is easy ; but the records are somewhat variable. Successive lifts may shorten or lengthen not simply in a regular but in a more or less irregular and periodic fashion, through temporary changes in the muscle, or in the nervous elements. If the variations in the load are made in an arithmetical series, the subject benefits by the suggestion of regularity ; if the presentation is irregular, he is often surprised and makes abnormal records. The usual procedure has been to make both a regular and an irregular series of changes. In the records here reproduced only the regular series are given. While these records are variable they are not so much so but that some fairly general features may be discovered.

How nearly the series of lifts or the corresponding fatigue curves would be the same under the different conditions that may exist, I am unable to tell precisely. Nearly all the experiments at my disposal (about 700 single series), were made with four young men between twenty and thirty, under what appeared to be normal conditions ; a few records were obtained from five or six others. The majority of the records agree with the illustrations given, but there are some exceptions. For example, one subject, who gave a nearly straight, though slightly convex-concave, fatigue curve in the winter, five or six months later gave a curve decidedly convex with an abrupt

ending. The maximum pull, the load and the rhythm were the same.

In any case the results must be compared with the unchanging geometrical standards discussed above, to enable us to say in what way the series of lifts or the fatigue curves vary in comparison with the shortening of the muscle, and so to state definitely what is taking place.

With regard to the series of lifts for the different phalanges, the outline of a series corresponding to regular gradations in the load is nearly a straight line in the case of the abductors and the flexors of the last phalanges, when the total change in the load is from the maximum to half the maximum, or the reverse. With the flexors or abductors, a convex is a more frequent deviation than a concave outline; with the extensors, the reverse is true.

In the fatigue curves at the right of each series of lifts (see the figures below), the load was one-half of the maximum; and the interval between lifts, two seconds. The fact that the fatigue curves were not taken at the same time as the series of lifts, except in the case of Figure 10, will explain the difference between the maximum pull in the lifts and fatigue curves in Figures 7, 8, 9. It will be seen that the outlines of the series of lifts and the fatigue curves correspond fairly well. I do not believe this relation is invariable, but just what differences occur or under what conditions they take place, I am unable to say.

Figure 7 has been given partly because the results are typical for the flexor profundus of any finger, within the limits indicated above and as far as the point X; and partly because of the striking recovery of power by the subject when he had nearly reached the point where he would be unable to move the load. Lombard's results were unknown to him and the recovery came as a surprise. Such pronounced recoveries of power have usually not occurred in the work of those who have assisted me. I have, however, several similar records from this one subject and small variations have been observed in all. In one other case, these recoveries became very marked for a short time, though they had not occurred at all in occasional experimenting for some years previous. Why they should be prominent in some and not in others, or why they should appear

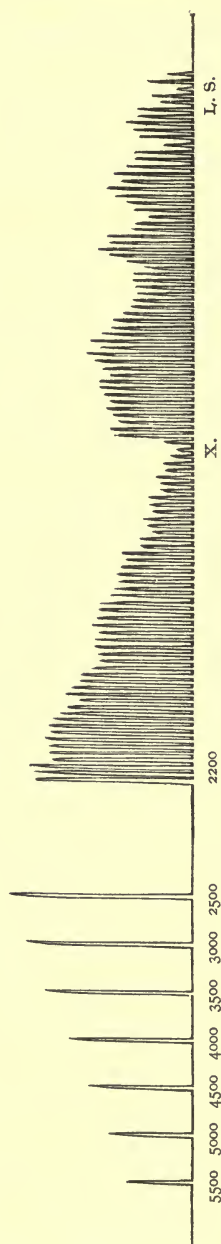
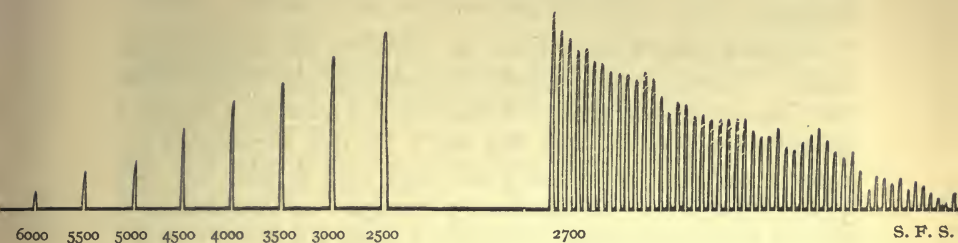
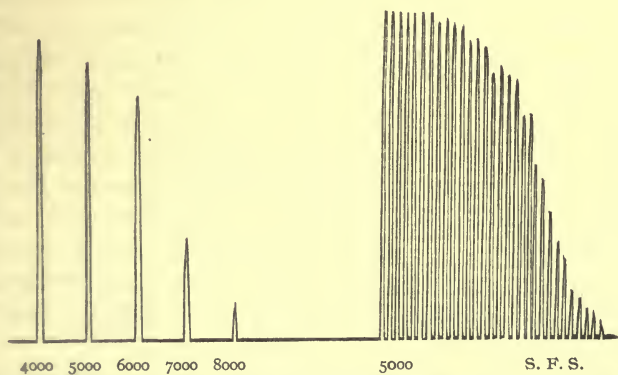
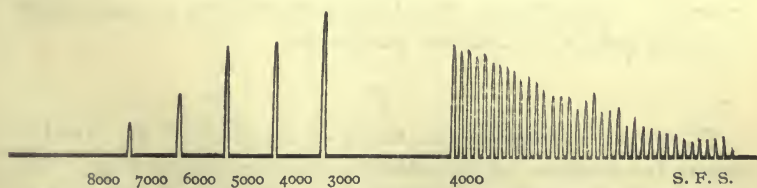


FIGURE 7. *Flexor profundus, first finger of right hand.*

FIGURE 8. *Abductor indicis, left hand.*FIGURE 9. *Flexor sublimis and flexor profundus combined. First finger left hand.*FIGURE 10. *Chiefly the flexor sublimis. First finger left hand.*

at one time and not at another in the same person under apparently similar experimental conditions, seems at present obscure.

Figure 8 gives a result from the abductor indicis of the left hand. Records from the abductors and from the flexors of the last phalanges have usually resembled one another. Figure 9 gives results differing from the rest. In this experiment the rotation occurs at the second joint, and a long thimble has been pushed tightly over the last two phalanges; we therefore have here the combined effects of the flexor profundus and the flexor sublimis.

Figure 10 gives the curve obtained from the same finger under similar conditions except that a short thimble has been pushed over onto the second phalanx leaving the third phalanx free to move. With this arrangement the series of lifts and the fatigue curve resemble those obtained from the third phalanges. It is probable that we have to do in this case chiefly with the flexor sublimis. As will be seen from the above records, the force of contraction decreases gradually with the flexion of a phalanx till it is only about a half of the maximum at the limit of motion. For the flexor profundus to be effective in bending the second phalanx, when the third phalanx is free, the latter must first be bent to its full extent and the muscle must be shortened still further. The force it can bring to bear will evidently be small.

In general, under the conditions mentioned, a fatigue curve from a single flexor or abductor is likely to be a long uniform curve approximately rectilinear, often slightly convex-concave, sometimes convex and abrupt, and occasionally a little concave; fatigue curves from the extensors are usually long and slightly concave. Experiments with the extensors may be made either with the last or the last two phalanges.

V.

Figure 11 illustrates the mode of fixation of the hand to secure the isolation of the abductor indicis of either hand. All the parts are completely adjustable so that experiments may be made with children as well as with adults; this is true also of the arrangements for the flexors and extensors. This experi-

It will be seen that the downward pressure exerted when the abductor indicis contracts is met by the platform at Y. The upward pull on the metacarpals of the first finger and thumb is resisted by the clamp X, while its upward pull on the first phalanx may give rise to whatever movement it can. These and the rest of the clamps keep all parts of the hand in a fixed position and prevent them from contributing to the movement of the first finger.

For experiments with the abductor of the little finger, the same system of clamps is used, but the instrument is inverted and the movement of the finger is made downwards. The point of application of the load is immaterial unless the finger bends under the strain, or pain is felt. The place usually selected is that directly over the second joint. The angular adjustment of the saddle R takes place by movements of A. This moves in two concentric slots, the center being the center of rotation of the lever. Therefore, whatever adjustment may be made, the pointer will always remain at the zero of the scale whenever the end spring is used. All the abductors are well exposed for electrical stimulation, especially so the abductors of the little fingers. The angle at which the phalanx begins to work is as important in these cases as in those previously considered, but I believe nothing more is necessary than to hold the hand straight forward and make all parts tight after centering, though a record may be kept of the angles used. For many experimental purposes the abductor indicis is very convenient; it is of medium strength, not liable to strain by ordinary work, and does not readily become painful.

VI.

The most important and most difficult part of ergographic experimentation has now been discussed. The second deals with the character of the load or resistance, and with modes of registration. The dynamometers and dynamographs, which preceded the ergograph, were spring instruments. Fick's tension indicator for experiments with the abductor indicis had a strong spring as a resistance, and was designed, as the name indicates, to register the tension of the muscle with a minimum of shortening on its part, that is, to leave the muscle practically

of the same length, or isometric throughout a contraction. Of more recent dynamometers, Kellogg's registers the pressure by the rise of a column of mercury in a tube of small diameter.

In the original ergograph, Mosso made use of weights instead of springs, partly perhaps because weights were customary in myographs for excised muscles, and he believed a similar reference to elementary properties of length and weight would be possible with the ergograph, and partly because it enabled him a little more readily to estimate the total amount of external work.

Criticism of the Mosso ergograph during the past ten years has not only pointed out the absence of the necessary isolation of the muscles employed, but has also been partly directed against the exclusive use of weights as a resistance. The inertia of the weights, the friction of the working parts, the failure of the apparatus as a whole to keep the contractions isotonic, the absence of definite means for selecting the load desired, the fact that the fatigue curve will end abruptly when the muscle can no longer lift a given load, and the lack of general convenience are among the criticisms that have been offered. It has also been asserted that the record of the external work executed does not represent even with approximate correctness the total amount of physiological work done. In raising a weight, moving it equal distances at different stages in the contraction, is not equally expensive; work also is done in sustaining or lowering as well as in lifting the weight. As a result several experimenters have preferred spring resistances, with which some of the above disadvantages, such as inertia and the abrupt ending of the fatigue curve, may be avoided; and a number of important observations and experiments have been added to the list.¹

Yet with a removal of the errors noted above, isotonic contractions have a special interest. They permit a comparison with a great deal of experimentation on excised muscles and they make a convenient standard if heights of contractions are

¹ Cattell: Science, N. S., V, 1897, p. 909; Vol. IX, 1899, p. 251.

Binet and Vaschide: Examen critique de l'ergograph de Mosso, L'Année Psychologique, Vol. IV, 1897.

Robert Müller: *op. cit.*

to be studied. The series of variations in height and tension, with a series of increasing or decreasing loads, together with the corresponding fatigue curves taken under many different conditions, certainly represent definitely measurable changes which it does not seem probable experimenters will soon neglect. In the use of springs, the isometric procedure of Fick represents uniform conditions, permitting the making of comparable records, which will doubtless prove to be of much value both for such purposes as those for which it was employed by Fick,¹ Schenk,² and Franz,³ and for the measurement of the maximum tension of a muscle in connection with other forms of experiment, particularly the isotonic.

Since the maximum force of contraction varies with the initial degree of flexion, it will be best to take as the standard measure the force exerted when the muscle is extended so that the phalanx makes an angle of 180° with the preceding part of the finger. To obtain this measurement something like the isometric method will be most convenient.

With springs of different degrees of extensibility the work that may be done or the tension that may be produced by a contraction will not be the same—the greatest tension being registered by the spring permitting the least, and the greatest amount of work by that permitting a medium, shortening of the muscle.⁴ While this renders results obtained by muscles of different strength with such springs difficult of comparison, nevertheless the use of them offers opportunities for several interesting experiments like those from which these conclusions were drawn, so that we may demand that an ergograph to be of general use must furnish means for these as well as for the more important isometric and isotonic experiments.

An especially important requirement is this, that it shall be possible to use all these different forms of resistance under exactly the same conditions, that is, with the same adjustment

¹Fick: *op. cit.*

²Schenk: Ueber den Verlauf der Muskelermüdung bei willkürlicher Erregung und isometrischem Contractionsact. *Pflüger's Archiv*, Vol. LXXXII, 1900.

³Franz: On the Methods of Estimating the Force of Voluntary Muscular Contractions and on Fatigue. *Am. J. of Physiology*, 1900.

⁴Franz: *op. cit.*

of the phalanx and its muscle in order that comparisons of records may be made with certainty.

Figure 12 shows the manner in which it is sought to attain these ends in the instrument here described.

For isotonic contractions we have the strong spring M S in the tube; it is variable in length and tension by a screw at the lower end and is supplied with a scale. The increment of tension occurring when the lever is moved is compensated for by the spring C S, which is therefore called the compensating spring. An account of a spring myograph is given by Dr. Grützner¹ in which compensation for the increment of a spring is obtained by adjusting it for each tension to a given angle with the lever to which the muscle is attached.

It will be seen that for the first half of the possible movement of the lever R, C S re-enforces the pull of M S with a gradually decreasing power till a zero point is reached near the middle after which it counteracts M S with a gradually increasing force. The compensation is of course not quite perfect but sufficiently so; the difference within the usual range of movement cannot be readily detected with a spring balance stretching 20 cm. per kg. and applied 10 cm. from the center of rotation.

It holds, whatever may be the tension of M S, and may be easily adapted to main springs of different strength; it also applies to the combined increments of M S and the coiled spring by which the pen is moved back.

For isometric contractions we have the spring Sp, the mode of attachment of which can better be seen in Figures 6 and 11. As was indicated above, it may be used not only for isometric experiments but also in obtaining the maximum force of contraction for the estimation of the resistance to be used in isotonic experiments. Experiments with springs of different degrees of extensibility may be made by using a dividing band or by putting other springs in the place of Sp.

The scales for M S and Sp, the main divisions of which are in hektograms, correspond in their units and are constructed with the lever in motion so as to include the effects of friction

¹ Dr. P. Grützner: Ein neues Myographion. *Pflüger's Archiv*, Vol. XLI, 1887.

and the resistance of other parts of the apparatus. The force of the springs is referred in the calculation of the scales to a radius of 31.83 mm. or a circumference of 200. It is by a sector of a wheel of this circumference that the graphic records are made on K K so that the amount of external work done is obtained in Hg mm. by multiplying the scale reading of M S or Sp, in the first case by the length, in the second, by half the length, of the up strokes taken singly.

The regulating mechanism of the little kymograph K K is made of the train of wheels and a cylinder escapement of a cheap stop watch; the rim of the balance wheel has been cut away so that what remains of the wheel vibrates very rapidly. It has been in use a great deal during the past five years but does not appear to be badly worn yet. A small fountain pen makes the record on a continuous roll of paper.

For isometric contractions, the length of the graphic record is multiplied ten times by pushing the wheel M P into gear. In this case, the spring that pulls the pen back to the right has such a mechanical advantage that the contractions start with a small initial load. Should it at any time be desirable to avoid this initial load, the pen must be disconnected and dependence placed upon a record secured by air transmission to an ordinary registering tambour and kymograph, from a receiving tambour placed at T M. This method has, however, a still more important use in that it makes it possible to study the character of single contraction curves at the same time that the fatigue curve is traced on K K.

In the first form of the instrument this mode of registration was used exclusively but it was liable to a great deal of inaccuracy and indefiniteness. The use of celluloid tambour tops, resembling the tops of aneroid barometers, such as are made by Sandström at Lund, would doubtless prevent the trouble due to deterioration of the rubber, which is the chief source of error.

S R is a device like Fick's Arbeitssammler and gives the sum in mm. of all the up strokes in fatigue curves, by the isonic method. In this kind of experiment it is only necessary to multiply the readings of S M by those of S R to have at once the external work in Hg mm. In the isometric experiments the strokes must be measured singly.

The interval between contractions, and the rate of movement with which each contraction is executed, must be made as nearly definite and uniform as possible. To do this simply with the aid of a metronome requires great care. Even if the stroke is begun at the proper time, the rate of movement and the amount of rest taken between strokes may vary greatly. Just how great a difference in fatigue curves can be produced by such variations I am unable to state. I believe quite a difference may be made.

Several rather definite plans may be adopted; the contractions may, for example, be made with the utmost rapidity, or they may be made with an effort to make the upstroke in the first half of the interval and the down stroke in the second half. This, also, is a definite procedure; but a great deal of energy is spent in relaxing slowly. Between these two methods lie a number of less definite ones, among them what seems with a two-second interval like the most natural of all. The lever is lifted somewhat rapidly, a special effort is made near the top so that more time is expended there, particularly in raising, but also to a less extent in lowering the load, which is then allowed to descend rapidly but not so as to strike hard on the arrest; there the lever is allowed to remain for about a quarter of a second, when the next contraction begins. With care this rhythm may be made fairly definite, but the same duration of contractions and relaxations must be maintained whatever may be the height of the stroke.

In the apparatus at the Carolinska Institute, already mentioned, this problem is solved by having two endless bands with cross bars running, one up, the other down, while the subject of the experiment must move a similar band running in the center so as to equal the speed of the band moving up in the contraction and the speed of the one moving down in the relaxation.

No special devices of this kind have been used in the present case, but considerable assistance in the teaching of the different rhythms has been obtained by moving the finger in the rhythm desired directly before the subject, while the time is kept by aid of a metronome. This makes a vivid impression and is easily followed. If not the best, it would certainly be a good

method to adopt in experimenting with the various factors of rhythm.

The plan of Kraepelin of relieving the finger in the return movement from all pressure, by having a mechanism to catch and hold the weight at the heights reached by successive lifts, makes the record of external work approach more nearly to what the muscle actually accomplishes and prevents the liability to change in the rate of lowering the weights, but it does not necessarily make the rate of raising the weight any more nearly regular. This may still be slow or quick or variable at different stages so that in the comparison of records the results thus obtained may not be any more accurately proportional to the facts than the others. In the apparatus at the Carolinska Institute a similar purpose is accomplished by having the weights raised or lowered, as may be desired, by electrical power.

The peculiar arrangements of the ergograph described in this paper would not prevent the application of such a device, nor does it seem to me that the mechanical work involved is especially difficult. It could be accomplished by mounting a quarter circle of a strong gear wheel on the axis to operate against two or three wheels so proportioned as to give considerable speed to the last. By applying an adjustable friction to this, and by having a ratchet in the system so that the upward movement would be free, it would, I believe, be fairly easy to relieve the phalanx of pressure on the return.

Whatever device may be employed, the important thing is to make the movements as nearly uniform in character as possible in the different lifts, both in the same and in different experiments, unless the matter of rhythm is itself the subject of study.

A number of other attachments, such as one to do the work of the Mosso ponometer, would not be difficult. This might be substituted for *A*, Figures 6 and 11.

VII.

For the purpose of a brief survey, ergographic experiments may be classified under three heads. Under the first we may place those made for the purpose of ascertaining the relation of

the shortening of the muscle to the force it can exert, or to the fatigue curve it will give under different conditions, and those for determining the seat of the fatigue or of periodic changes of power.

One of the most striking results in this field, obtained in the last few years, is the discovery that with a certain interval and the proper reduction in the load, a certain load is found that may be lifted to about the same height for a considerable period. A somewhat similar experiment may be made with springs as the resistance (in this case the heights of the strokes representing the pressures) will fall till a certain level is reached, which may then be maintained for a considerable time. If the interval between the first contraction and the beginning of the level phase had been found to be fairly constant in the same persons under apparently similar conditions, it might not have proved to be a more significant fact, but it would probably have been a more important individual measurement; this, however, does not seem to be the case.

Another series of experiments must also be mentioned under this heading.¹ From results obtained by alternate electrical and voluntary stimulation of muscles, in which it appeared that, when the muscle was fatigued for voluntary stimulation, it could be made to act by electrical stimulation, and then again by voluntary effort, some of the early experimenters advanced the theory that the fatigue and recoveries in the ergographic experiment were due to the fatigue and rest of the nerve cells. This theory led naturally to the view that the Lombard periodic curves were produced by central changes.

Treves reports that corresponding records may be obtained from the stimulation simply of the muscle itself, the periodic curves being due to an automatic lengthening or shortening of the muscle. Moreover, he argues that in alternating voluntary and electrical stimulation, the electrical stimulation may be regarded as submaximal and also as different in kind from the voluntary, both conditions permitting such recuperation as is

¹Dr. Zach. Treves: Ueber den gegenwärtigen Stand unserer Kenntniss, die Ergographie betreffend. *Pflüger's Archiv*, Vol. LXXXVIII, 1902.

Schenk: *op. cit.* T. J. Franz: *op. cit.*

found to take place. We have thus a thoroughgoing reference of a number of changes, which have heretofore been thought to be of central origin, to peripheral sources.

This does not, however, show that changes in force, even rhythmic changes, may not also occur in the nerve centers stimulating the muscle; from the close correspondence between muscle and nerve tissue in many elementary properties we might in fact expect this to be true. A certain class of psychic factors certainly affect the records; competition and encouragement often greatly augment both momentary strength and endurance, and discouragement or a sense of failure may produce the contrary effect. By pretending to present a subject with a series of increasing loads, successive reductions in the record may sometimes be observed, while as a matter of fact the load remains constant. Habits of effort, like habits of sleep, may no doubt exist or be established by training, and even variations like the Lombard curves are not impossible from such a source, but might be due to overcoming a reflex tendency to rest, just as we may by persistent effort counteract a tendency to fall asleep. Some variations in the ergogram are therefore clearly traceable to central and even mental factors, in others, these and peripheral factors may co-operate, and in still others peripheral factors may alone be responsible for the results.

In the second group of experiments we will place those in which the ergographic record is made a test of some general physiological condition. This field has been especially attractive, chiefly, perhaps, because of the direct practical inferences that may be drawn from the results. In this way the effort has been made to ascertain the effects of lack of food, loss of sleep, forced marches, mental fatigue, the effect of sugar, tea, coffee, alcohol, and tobacco, of diseases, and of some atmospheric conditions, like changes in the density of the air, also the effect of age, of time of day, and of various re-enforcing or depressing mental states.

Perhaps the most generally interesting and important recent question¹ in this field has been whether the ergograph may not

¹T. L. Bolton: Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut und Muskelleistung. Kraepelin, *Psychologische Arbeiten*, IV Band, 2 Heft. 1902.

be used as a means of studying the effects of fatigue, especially mental fatigue, in school children, as has been attempted by Keller, Kemsies, and others. That the ergographic record will not be as large as normal in conditions of great general exhaustion resulting from loss of food or sleep, from disease, or from excessive mental or physical labor, we may certainly take for granted even without the evidence offered by Mosso and Maggiora. Moreover, certain other aspects, such as regularity, ease of recovery, and the general outline of the curve, may prove to be as significant as the quantity of work in this matter. If a condition of general weakness were produced by work of any sort in the course of the day or week, and the general re-enforcement that accompanies effort had disappeared, we may feel confident that the fact would be revealed in the ergographic curves. But even in doing the same kind of work there is for a time an increase in the quantity done. Some observations would indicate corresponding variations with the amount of energy available from the start in the record of any test that may be applied, providing it does not require a change in interest or association. Thus, in the experiment with Dr. Adduco, Mosso found during the progress of exhausting mental work at first an increase, then a decrease, in the ergographic record. In experiments to determine the daily variations in rate of work, the highest records are usually not made early in the day, and the quantity of work in the addition tests applied by Laser¹ at the end of several successive hours of school work increased for the first three or four hours.

The ergographic record changes also with the emotional tone so that there are a number of stimulating and depressing factors that must be considered in using it as a test of exhaustion. Moreover the amount of change produceable in a test by fatigue is in proportion to its difficulty and the ergographic experiment is only moderately difficult.

The problem of the local and general character of fatigue must also be considered. While we can suppose that the poisonous condition of the blood in exhaustion, shown by Mosso to exist in the case of dogs, will be most detrimental in

¹Hugo Laser: Ueber geistige Ermüdung beim Schulunterrichte. *Zeitsch f. Schulgesundheitspflege*, 1894.

the locality where the poison is produced, yet in the cases reported there were marked general effects. The reflex nervous effects tending to increase or inhibit work are probably also partly local and partly general. A third effect of work, namely, consumption of cell material, we may suppose to be more local in character.

Muscular fatigue at least is largely local; and the experiments of Weygandt, which show that under laboratory conditions there is no recuperation, as measured by the rate of work—in changing from one kind of mental occupation, such as adding or multiplying, to learning nonsense material by heart—unless the change was to something easier, do not seem to exclude the possibility of the specific local, as well as general character, of mental fatigue, inasmuch as the work to produce fatigue was continued for only a relatively short time. The conclusions may not apply to the exhaustion produced by continued application to a given subject till work with it becomes painful and we at least feel that it is necessary to take up something else for a time.

There may also have been such a balance in the stimulating and depressing nervous factors that the partly local character of the fatigue was masked for a while, and perhaps would be so in work of corresponding difficulty and duration in ordinary life. With these many factors to obscure the result and with fatigue partly a local phenomenon, we cannot expect to use the ergograph as a measure of exhaustion in the same way as we use a thermometer to measure temperature, but it may do a very important service in aiding us in its field of application to analyze the conditions and effects of work and fatigue, and in extreme cases, no doubt, if we need a test of exhaustion, to serve as one.

In the third class of experiments may be placed those in which the purpose is to aid in solving practical problems of work and training by making analogous tests with the ergograph. Such experiments would be those made upon the relation of the load to the maximum work,¹ the relative detriment of work at the beginning or near the point of exhaustion,

¹ Maggiora: Ueber die Gesetze der Ermüdung. *Du Bois-Reymond's Arch.* 1890.

the effects of various intervals between lifts, sustained and rhythmic work, cross training, the effects of warming up, and the conditions of recovery. This section of ergographic work will not be complete till a solution of all the elementary problems of neuro-muscular training and work has been attempted, and, as yet, only a few researches are at hand.

We must, of course, admit at the outset that experiments with small isolated muscles will not give results wholly and directly comparable with the training of different muscles or muscle groups. In some cases work may be so general and severe as to be limited more in character by circulation, respiration and digestion, than by the work of the muscles themselves. Yet the very fact of isolation from general disturbing factors, the possibility of definite variation and measurement, the relative ease of obtaining results, and the need of knowing in the analysis of neuro-muscular work what elementary factors are involved, are the very conditions which assure us of the permanent value of this kind of experimentation.

ATTENTION WAVES AS A MEANS OF MEASURING FATIGUE.

By Professor W. B. PILLSBURY, University of Michigan.

Early in 1902 the work of Dr. Slaughter and of Mr. Taylor,¹ and in particular the first paper of Wiersma,² upon the influence of short periods of work on the fluctuations of the attention, suggested that the same method might be applied to comparing the condition of fatigue at different times of the day. As the writer found himself compelled to do an unusual amount of teaching in the second semester of that year, it seemed that some interesting results might be obtained by taking a record of the attention waves in the morning just after beginning work, and in the evening before dinner. While living in Würzburg the following summer the records were continued, through the kindness of Professor Külpe, who placed his laboratory at the writer's disposal. Later, Professor Külpe volunteered to take a day's record. Again, after returning to America last fall, the writer continued the experiments upon four men.

Wiersma's second article,³ which anticipated our results so far as experiments upon the daily rhythm of the attention waves is concerned, was received after our preliminary experiments were well under way. It nevertheless appeared to be worth while to continue the investigation, as Wiersma's results were obtained from but few persons and covered but three days. Then, too, he paid no attention to the length of the wave, and one of the main purposes of the present investigation was to discover if the waves had a daily rhythm in length that is analogous to the diurnal change in the rate of most physiological rhythms.

The apparatus used here consisted of a long horizontal drum,

¹ *Am. Jour. of Psych.*, March, 1901.

² *Zeitschr. für Psychologie*, XXVI, p. 168.

³ *Ibid.*, XXVIII, p. 179.

with the carrier for the time-markers driven by an endless screw. The drum, when moving at a rate of a centimeter to the second, gave an unbroken record for 14 minutes. The drum was driven at a constant rate by an electric motor, but as a further precaution, and for convenience in reading, the time was recorded by a Jacquet chronograph, which wrote fifths of seconds. The observer sat in the dark room facing the Masson disk, and at a distance of 55 cm. from it. The disk was illuminated by a 16 c. p. lamp at a distance of 20 cm. and carefully screened from the eye of the observer. When the writer was subject no experimenter was present. The motor was started by reaching from the dark room after everything had been made ready and the record continued until it seemed that sufficient time had elapsed to fill the drum. The disk was the same for all subjects except P. and gave a ratio of black to white of $\frac{1}{32}$, for P. $\frac{1}{40}$.

In Würzburg the waves were written upon the Zimmermann kymograph with the automatic lowering device. The apparatus was set up in the darkroom; the disk, at a distance of 140 cm. from the observer, was illuminated by a Welsbach lamp. During a large number of experiments Professor Külpe took charge of the apparatus.

When the records of Dr. Wallin and Mr. Freund were taken the drum used for the other experiments here was required for another investigation, so that it was necessary to use a simple kymograph. This was the least satisfactory of any of the methods employed, as the rate was so slow that only seconds could be recorded, and in reading, these must be estimated to tenths.

The observers were Professor Külpe (K.), Dr. Wallin (W.), Mr. Hayden (H.), Mr. Galloway (G.), Mr. Freund (F.), and the writer (P.).

The investigation furnishes material for the discussion of three problems: (1) the degree of fatigue at different times in the same day as influenced by the amount of work that has been performed, and by the type of the individual; (2) the course of the fatigue that is induced by the work of recording the attention waves as affected by the time of day, and (3) the fluctuation in the length of the total wave with the time of

day. Instead of always taking records of the same length, as Wiersma did, and using the total period of visibility or invisibility as the measure, we permitted the length of the record to vary, within limits, and used the ratio between the period of visibility and the period of invisibility as the index. In working up, the curves were divided into five or six equal divisions, and each part was averaged separately first, and then these several sums were added and treated as wholes. In preparing the tables for publication, the corresponding times of the days that were of the same character are averaged together. Except for *G.* and *F.*, each day's curves showed the same tendency as the average. It seems hardly worth while to take the space to give the results for each day, and the average is a fairer way to represent the results as a whole, than to select sample days.

Experiments were usually made about 9.00 A. M., at noon, just after the noon-day meal, and in the evening. The tables show that we may divide our subjects into four distinct groups. *K.* is a typical evening worker. There is a progressive increase in his ability to see the gray ring from the first experiment in the morning, throughout the entire day. It is true that we have but a single day's results, but Professor Külpe had worked with Lehmann and others in the early investigations at Leipzig, and was a highly trained observer in work of this kind.

P. and *W.* are just as typical morning workers. Both show a continuous increase in capacity up to the evening curve, with a decrease in that. *W.* is particularly interesting for us, because his fluctuations are of very short period, and for the most part correspond in length to the respiratory rhythm, with only an occasional longer wave. His results show that attention waves of this type can also be used to discover the status of fatigue of the individual. *F.*, too, is probably of the morning type, both from what he says of his habits of work and from our records. One day's evening record, however, is inconsistent with this statement; the ratio of visibility to invisibility rises from 1.50 in the morning to 5.08 at 6.00 in the evening, without anything in the records of the daily routine to account for it. On the other two days, and in the average, his results agree with his statement of his methods of work.

TABLE I.
K. August 11, '02.

Time of Day.	No.	m V	m I	$\frac{V}{I}$	Sum.
9.00- 9.30	38	7.8	5.7	1.6	13.5
12.00-12.45	42	9.5	4.9	1.9	14.4
2.55- 3.10	41	11.3	3.3	3.4	14.6
6.00- 6.22	33	16.4	2.9	5.6	22.0

The column headed *No.* gives the number of waves measured; that headed *m V* the mean of the periods during which the ring of the disk was visible; that headed *m I* the mean of the periods during which it was invisible; that headed $\frac{V}{I}$ the ratio of these two periods; and that headed *Sum* or *V+I* the total length of the attention wave. The lengths are given in all cases in seconds.

TABLE II.
G.

Time.	No.	m V	m I	$\frac{V}{I}$	V+I
Morning 9.00	249	4.8	4.1	1.17	8.9
Noon	181	5.1	3.8	1.35	8.9
After Dinner	265	4.8	4.1	1.17	8.4
Evening	377	4.6	3.8	1.21	8.4

TABLE III.
H.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	287	7.3	2.1	3.43	9.4
Noon	230	6.3	2.4	2.63	8.7
1-2 P. M.	285	7.2	2.1	3.35	9.3
Evening	300	6.7	2.3	2.94	9.0

TABLE IV.

W.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	275	3.3	2.1	1.55	5.4
Noon	196	3.9	2.4	1.64	6.3
Evening	281	3.0	2.1	1.46	5.1

TABLE V.

F.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	73	15.5	3.4	5.03	18.9
Noon	37	15.0	3.2	4.66	18.2
I P. M.	79	10.9	2.5	3.44	13.4
Evening	63	15.4	3.4	4.53	18.8

H. belongs to neither of the Kraepelin groups, but has two maxima and minima each day. He both tires very quickly and recovers very quickly, so that his morning and his early afternoon records are alike, and his noon and his evening records. The rest at noon, without sleep, almost completely removes all evidence of his morning's work. This result agrees fully with his own statement, made before the curves were worked out.

G. is entirely irregular. His capacity varies differently for different days, and in the average it would seem that his morning and evening conditions are very much alike. He had not noticed any particular difference in the ease of work at the various times of day, so that even in this case there would be no lack of harmony between the results of the records and the man's own account of his working habits. It is to be remarked that *F.* and *G.* are the youngest of the persons experimented upon, and it would be quite in accordance with

Wiersma's results to assume that they had not yet acquired fixed periods of work.

The only chance to study the influence of different kinds and amounts of work is offered by *P.*'s records. All the others were taken on days of about the same routine, or at least the differences are not sufficient to render a decision certain. The records from *P.* cover three distinct kinds of life. Four records were taken on days when he was engaged in teaching from seven to nine hours a day. This work was usually divided into three hours lecturing, two hours of discussion in seminary classes, and from two to four hours of directing laboratory work with classes large enough to engage his attention pretty thoroughly throughout the time. The physical exertion of talking and standing, with the mental work involved, usually left a feeling of exhaustion at the close of the day. Three records were from days during vacation, which were spent in writing three to five hours of the day, with an hour or so of exercise a little before the evening test was taken. A comparison of the records in Tables VI and VII, shows that on the average there is a decided advantage in the evening records of the easier days, and that, too, in spite of the fact that the morning tests show a much greater capacity on those days than on the hard working days. One would expect that it would be much more difficult to produce a decline in efficiency of one half when the efficiency was already low, than when it was comparatively high. The records of March 25 were unusually high as compared with other days of the hard working period. Examination showed this to correspond with the fact that the entire morning of that day was spent in laboratory work, instead of two hours in the laboratory and two in lecture, as on the other days. Evidently, then, so far as can be judged from a single day, lecturing is more fatiguing than the direction of laboratory work, and this result probably corresponds with the experience of most men.

One record, taken during the vacation period, was found to have been made on a day of more than usual indulgence in physical exercise. An afternoon was devoted to golf, and it was entered upon the record that the subject had begun to give out and to play badly before the end of the last round. As

TABLE VI.

P. Hard Days, Mch. 26, 28, Apr. 10, May 1, '02.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	145	9.8	3.6	2.52	13.4
Evening	230	4.4	3.6	1.23	8.0

TABLE VII.

P. Vacation Period, April 11, 15, 16.

Time	No.	m V	m I	$\frac{V}{I}$	Sum.
Morning	116	11.3	2.7	4.10	14.0
Evening	163	7.7	3.1	2.88	10.8

TABLE VIII.

April 17. Physical Fatigue.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	42	16.5	2.9	4.10	14.6
Evening	58	4.6	1.9	1.61	6.5

TABLE IX.

P. March 25, '02.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	22	11.7	2.9	4.10	14.6
Evening	38	9.6	3.0	3.20	12.6

will be seen in Table VIII, there had been a decline in the ratio of the attention wave from 4.10 to 1.61, as compared with 4.10 and 2.88 for the moderate days.

A comparison is also possible between the days of the quiet

TABLE X.
P. April 7, '02.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
Morning	37	14.3	2.6	5.85	16.9
Evening	51	8.4	3.0	2.77	11.4

TABLE XI.
P. July 11-19, '02.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
9.00-10.00	300	6.8	2.8	2.43	9.6
11.30-12.00	245	6.1	2.4	2.51	8.5
3.00-4.00	337	5.9	2.3	2.56	8.2
6.00-7.00	365	5.1	2.4	1.62	7.5

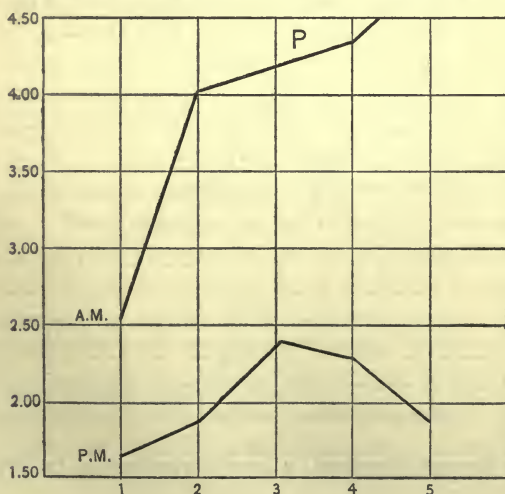
TABLE XII.
P. July 17. Slept between 2 and 3.

Time	No.	m V	m I	$\frac{V}{I}$	V+I
9.00-9.20	58	5.38	2.23	2.41	7.61
3.05-3.35	63	4.93	2.25	2.19	7.18
6.15-6.40	73	5.05	2.39	2.32	7.44

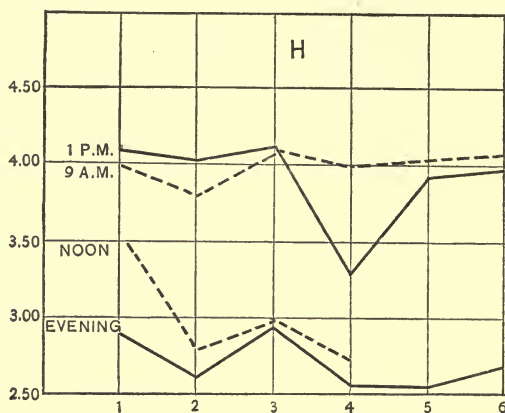
life in Würzburg and the more active life here. While in Würzburg the mornings were usually spent in attending two or three lectures at the university, and in working on curves. The afternoons were devoted to working on the curves, or to an occasional short walk. While the results are not strictly comparable, both because the disk used had a ratio of black to white of $\frac{1}{45}$, as compared with $\frac{1}{40}$ here, and because of the different distance and illumination of the disk, nevertheless the ratios between morning and evening results may be com-

pared. It will be seen from the tables (VI-IX as compared with XI) that the difference for the two periods corresponds closely to the nature of the work done. One day, July 17, shows another form of correspondence. Contrary to the usual habit, an hour's sleep was indulged in on that day between the afternoon and evening records. As a result it will be seen, Table XII, that the evening record is very nearly like the morning record, more like it, in fact, than the afternoon record. So far, then, as our results show anything at all as to the relation between the amount of work and the ratio of the period of visibility to invisibility, there is an inverse relation, as would be expected from theoretical considerations.

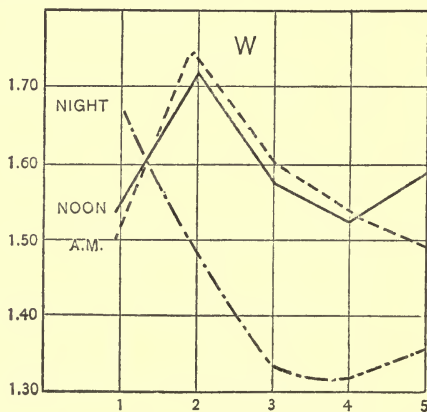
If we divide the series taken at any time of day into smaller portions, and average each part separately, it will be possible to study the advance of fatigue during any one of the daily periods. The results from *G.*, *H.*, *W.*, and *P.*, are susceptible to this form of treatment. The results from *F.* were in too short series to make this consideration possible, as were also all the results obtained in Würzburg. *G.*'s curves show as little change in this respect as in the daily rhythm, however, and so may be omitted from consideration. The results from the other three men are plotted and shown in the figures below. It will be seen that the results here, harmonize in every way with those of the period as a whole. For *P.* there is a constant in-



crease in efficiency in the morning curves, while in the evening the initial increase, due to exercise, is soon followed by a decline. For *H.* the early morning and afternoon curves are



pretty well sustained except for fluctuations throughout. If the fluctuations have any meaning, they would mean an initial decline before the warming-up process begins, then a second fatigue period, followed in turn by catching the "second wind," which results in making the beginning and the end of the curve very much alike. In the evening curves the minor fluctuations show themselves to a slight extent, but are entirely overshadowed by the continuous decline. *W.*'s two morning



curves are almost identical, a long warming-up period followed

by a longer decline. The evening curve, on the contrary, shows a marked decline throughout. In all, however, we find exactly what we should expect from the other tables, that the periods of general fatigue are accompanied by rapid exhaustion, while the period of increasing efficiency due to exercise (*Uebung*), so prominent in the rested condition, rapidly disappears.

The third question in mind when the investigation was begun was whether the length of the attention wave as a whole, the sum of the visible and invisible periods, had a regular daily variation. Dr. Slaughter found that the attention wave corresponded in length to the Traube-Hering wave of blood pressure. His results have recently been confirmed by Bonser in ignorance of the existence of the earlier work. If the hypothesis be true that there is a real connection between the two, it would seem probable that the attention wave has a diurnal variation, as have the pulse and respiratory rhythm. The last column in each of the tables brings together the evidence on this point. It will be seen that for all the different persons the greater length of wave corresponds to the time of greatest attention efficiency. For the morning workers there is a constant decrease in the length from the morning through to evening, while *K.* shows an increase in length between morning and evening. *H.* again shows the longest waves early in the morning and in the afternoon, with a shorter one at noon and night. The difference for *F.* is very slight on the average, so slight that taken alone it could have no meaning. This, however, is due to exceptionally long waves on the day when he showed his greatest efficiency in the evening. *G.*, on the other hand, joins the ranks of the morning workers in this respect, and shows a considerable quickening of the rate in both the afternoon series. It might seem strange that *W.*, whose attention wave more nearly corresponds in length with the breathing rhythm, should show the same daily variation as the individuals whose waves are of the Traube-Hering type. But it must be remembered that the breathing is also normally quicker in the evening, and the pneumograph records taken at the same time as the attention waves shows that the normal quickening occurs in his case. As *W.*'s attention wave is in part the cor-

relate of the Traube-Hering, in part of the respiratory, rhythm, so the quickening is probably due partly to the change in each.

In an investigation soon to be published, Mr. Galloway has found that the Traube-Hering waves actually show a diurnal periodicity, corresponding to that here noted in the attention waves. That the attention waves and the vaso-motor waves have the same daily variation, adds another link to the chain of evidence that the two have the same physiological basis.

We can explain our results if we consider the fluctuations of the attention a resultant of two physiological processes, of the degree of efficiency of the cortical cells, on the one hand, and of the state of excitation of the vaso-motor center on the other. The reinforcement from the medullary center would have its effect in decreasing and increasing the response of the cells, and would determine the rate of the fluctuation, but the proportion of the cycle in which they would be sufficiently effective to give rise to a sensation, would depend primarily upon the freshness of the cells themselves. The degree of efficiency of the cells, then, would be measured directly by the ratio of the period of visibility to the period of invisibility of our minimal stimuli, while the length of the total wave would be a measure of the length of the Traube-Hering wave.

In conclusion, I desire to express my thanks to the persons who so kindly gave their time to the work, and particularly to Professor Külpe, both for the facilities of his laboratory, and for his advice and encouragement, as well as active co-operation in the work.

STUDIES IN PITCH DISCRIMINATION.

By Dr. GUY MONTROSE WHIPPLE, Cornell University.

In earlier articles¹ I have presented an exhaustive analysis of the process of pitch memory and pitch discrimination as conditioned especially by the individual observer and by varying time-intervals. The eight observers who took part in those experiments varied distinctly in their musical attainments, as was shown by a preliminary report of their 'musical history' as well as by their subsequent work in the experiments themselves. At least three of the observers were musicians of some ability; two, on the other hand, were quite unmusical. At the same time it was a matter of regret that the individual variations were not more pronounced.

More recently the opportunity has offered itself to examine two individuals, one of whom has the 'gift' of absolute pitch, the other of whom is a typical case of marked 'unmusicalness.' The first two of the following studies are concerned with these two cases.

The third study is likewise an outgrowth of my former articles.² It is an attempt to investigate somewhat briefly, in the case of an observer of musical ability, the memory and pitch discrimination of chords and melodies as compared with isolated clangs.

I.

The observer in the first study, Miss M. C. Meyer, a Sophomore in Cornell University, comes of a musical family, though her parents are not musicians. Her mother, however, sings somewhat; an older brother plays well by ear; a younger brother is almost tone-deaf; she, herself, of course, is very musical.

¹This *Journal*, XII, July and October, 1901, 409, and XIII, April 1902, 219.

²See especially *loc. cit.*, XIII, 268, the concluding paragraph.

This rather striking unevenness in the capacity of the family, especially in that of the children, all of whom presumably lived in nearly the same musical environment, suggests that the absolute pitch capacity is a specific, constitutional tendency, a specific extension of the general musical *Anlage*, which, in this case, was inherited by *M* as a variation from some of the musical ancestors, while her younger tone-deaf brother inherited no musical nature at all, resembling rather his parents and other ancestors.

This question of the source of musical ability and non-ability does not seem to me to be conclusively settled. And even if, as just suggested, we attribute the ability at bottom to a specific, transmissible nervous tendency, we do not preclude the possibility of greatly modifying this tendency by post-natal training, whether favorably or unfavorably. M. Meyer¹ has shown, for instance, in regard to this particular phase of the subject, how a very fair absolute pitch memory may be acquired by dint of insistent practice. I believe, however, that this would be impossible without an originally musical 'disposition.' We shall return to the general question in the second study.

M sings soprano, but has never received vocal instruction. On the other hand she was taught to play the piano at the age of eight, took lessons for a period of six years and has played considerably ever since. She also plays the violin a little. It is unfortunately impossible to determine more exactly the nature of *M*'s early musical training where it would seem most likely that the foundations for the exercise of absolute pitch memory must have been unconsciously laid, for the capacity was already well developed when first discovered by *M* at the age of 12. Though Köhler's method was first employed, two other teachers followed, each with his own particular method.

An examination of *M*'s musical imagery shows no trace of colored hearing, save that lively music is bright, like a flood of light, while depressing music is dark. Music is emotional, full of feeling, such that it might possibly under some conditions suggest colors, but none are ever actually seen.

Centrally excited musical imagery of various sorts is extremely prevalent. "I live to tunes some days." The whistle of the wind, the rumble of a heavy wagon, the confused murmur of a room full of people and various sounds in nature are full of musical notes for *M* and are constantly suggesting melodies to her. Most of the imaged music, however, is reminis-

¹ *Psychological Review*, VI, Sept., 1899, p. 514.

cential, not constructively created, being, for the most part, confined to melodies recently heard. If the source is in piano selections which *M* has herself played, the imaged music remains in that form; all other music is commonly translated into terms of *M*'s own voice. In trying actively to image a piano selection which she has herself played, *M* sees the score vaguely and also, just at first, the keys of the piano; besides this the finger movements are vaguely felt and the music is distinctly heard. In trying actively to image a selection which she has just heard, but has not herself played, *M* hears the music very distinctly and almost always tries to think of the finger movements. In general, the association of finger movements (motor, with or without visual accompaniments) is quite strong, so that, for instance, in listening to an organ recital, *M* is apt, or at least is able, to think all the finger movements that are being executed by the organist. This capacity seems one of the essential features of her absolute pitch memory.

Absolute pitch memory. When about 12 years old, as I have said, *M* discovered quite accidentally, while playing the piano in some school exercises, that she could assign the name to notes that she did not see played. Soon this capacity extended itself to a recognition of the pitch of other musical instruments and of the singing voice, though the piano was, and still remains, easiest to recognize, the violin next, while other instruments, especially those of unaccustomed color, and voices are more difficult.¹

In all cases the recognition is immediate—*i. e.*, not based upon any process of comparison or computation—and rapid. In all cases the recognition is in terms of visual and motor (tactual) processes; when a note is sounded there is an immediate visual picture of from two to five piano keys coupled with imagery of a motor sort, either of eye movement or of arm and finger movement (always of the right hand) toward a particular one of the visualized keys. In other words the

¹ Unusual tone colors, in other words, demand a special practice; if a stranger sings, it is not always possible to judge the absolute pitch of the notes sung at first, but after listening awhile, the association can be made with some exactness. The same thing holds true of pianos of different timbre; *M* can pass judgment upon them better after having played them for a few minutes.

recognitory judgment is a process like that of looking at, or more often of striking, a note upon the piano, save that it is all in terms of centrally-excited, instead of peripherally-excited, sensations.¹ The name of the note (auditory-articular) comes later as an association with the processes just mentioned.

There is never any confusion of the octave of the note sounded.² The motor accompaniments determine this part of the judgment. High and low regions are more difficult to judge, not in respect to the octave involved, but in respect to the particular note within the octave. The two best octaves are the once and the twice accented,—*i. e.*, those in which melodies are most played.

EXPERIMENTAL.

The accuracy and other characteristics of *M*'s absolute pitch memory were tested by a series of experiments with the piano. Sensible discrimination was tested by the Stern tone-variator actuated by compressed air.³

Pitch memory. In the short *preliminary series*, testing various octaves, emphasis was laid upon the introspection of the process of recognition. Of the qualitative results, the directness of the judgment and the prominence of visual-motor elements have already been mentioned. In addition the following points were established. (1) The interval sense is not used at all, even when conditions seem to suggest its aid, *e. g.*, giving notes separated by an octave or a fifth, etc., in immediately successive tests. In fact, *M* is not at all accurate in her designation of intervals. (2) Relatively high or low notes are not visualized as distinctly as those in the middle region. This is due, *M* thinks, to the fact that, in actual playing, she glances less often toward the outer octaves. (3) The speed of judgment is correlated with its assurance and its accuracy, *i. e.*, a rapid judgment is usually correct and given with assurance;

¹ Thus when *e'* was struck, *M* saw at once the keys *c'*, *c'*-sharp, *d'*, *d'*-sharp and *e'*, while *e'* 'stood out' (motor determinants) as the particular one sounded.

² Within the limits of the piano key-board.

³ By means of a device designed to furnish an air blast of uniform intensity which I have previously figured and described in this *Journal*, XIV, Jan., 1903, p. 107.

slow hesitating judgments are usually incorrect and given without assurance.¹

Of minor interest are several curious beliefs of *M*'s (not substantiated by the tests), such as that white notes are easier to recognize than black, that black keys have a smoother sound than white, that *b*-flat and *f*-sharp are easier to judge than the other black notes, that *d*-sharp is the most difficult note of all, that certain notes, in whatever octave they are found, have characteristic 'feels' quite aside from their place in the tonal continuum; *e. g.*, *f* in the small, the once and especially in the twice accented octave, is quite unpleasantly grating. This is the only hint of any peculiar mechanism for individualizing and remembering particular pitches. It does not appear, however, that *M* uses these supplementary associations at all in the process of naming the note, though, as we shall see, she is positive that they assist her in other ways. They are, in my opinion, to be regarded as purely incidental associations, themselves dependent upon the tonal memory, though they illustrate in a way the highly individual character that pitches may possess in the mind of a person who has absolute pitch.

Quantitatively the preliminary tests resulted in 70% correct judgments. Aside from a single mistake of a fifth, the errors did not exceed a major second, and the majority were minor seconds. It will be clear from this and subsequent tests that *M* does not by any means belong to that class of individuals who possess extraordinary fineness of absolute pitch memory. As Abraham has lately shown,² it is not uncommon for individuals to be able to detect, from memory, variations in the pitch of the *a* of small fractions of a semitone,—in his own case of four vibrations plus or minus.

In the *second series* of 75 tests confined to the once and the twice accented octaves the same point is brought out, for but 48 (64%) were judged correctly, 17 were too high, 10 too low. Of the errors 21 amounted to a semitone, six to a tone. Of the

¹This intimate correlation between these three factors corresponds entirely with what I have invariably found true in examining the judgment process. *Cf., loc. cit.*, XII, 445-6.

²Das absolute Tonbewusstsein. Psychologisch-musikalische Studie. *Sammelhefte d. intern. Musikgesellsch.* Berlin, 1901.

21 semitones, again, 19 were errors in which *M* designated an adjacent black instead of a white key. This is one of numerous bits of evidence to which we shall call attention, all of which show that *M* is an observer of a decidedly subjective type,—suggestible,—given to unconscious prejudice and bias. These erroneous judgments of black keys for white ones, for instance, ensued after she had tried the experimenter's piano, commented upon its 'mellowness' in comparison with her own, and stated that this smoothness made all the notes seem like flats. More particularly when, at the 12th test, some remarks were made about the recognition of the black keys, *M* assigned a black key in every one of the next 12 tests.

It must be borne in mind that all of the above experiments were made upon a comparatively unfamiliar piano whose notes seemed to *M* not only less brilliant but also less distinctive in character than those of her own.

Accordingly the *third series* of tests was made upon *M*'s piano. The results (for 100 judgments in the once and twice accented octaves) entirely confirmed *M*'s belief that she could give more accurate answers with this instrument. 92 judgments were correct. The eight errors were equally divided between too high and too low, and equally divided between major and minor seconds.

A *fourth series*, conducted by *M* upon her own piano, showed that fusions of two, three and four notes in the two octaves previously used could be correctly analyzed and named, note by note, in approximately 85% of the trials. The errors, as a rule, affected but one or two notes in the chords.

Abraham has shown by statistics that only 35% of those who boast absolute pitch memory are able to reverse the usual process, *i. e.*, to ideate or sing a note whose name is given. He adds that these are the individuals whose pitch memory is especially strong, rapid and capable of fine discriminations. We have seen that *M*'s memory is not extraordinarily fine; she is liable to err from a half to a whole tone in from 8 to 35% of the trials (depending largely upon her familiarity with the instrument employed). On the other hand she is able, at will, to ideate pitches with practically the same degree of accuracy. In a few cases the imaged note varied from the true pitch by a

half-tone, never more; in most cases there was no appreciable error. *M* believes that she is aided in this process, much more than in the tests of the first sort, by the fact, already mentioned, that each note has some peculiar characteristic. "The note *f* has a peculiar, harsh, grating quality which cannot be mistaken; *b* has a sad, melancholy sound; *c* a soothing, restful quality, *e* a lively, cheerful sound," etc.

Sensible Discrimination. Tests of qualitative sensible discrimination were made with the idea of seeing whether *M*'s absolute pitch memory would be of any aid in the detection of fine tonal differences. Incidentally this work developed several points of interest in regard to the use of various methods with an observer of the subjective type.

The method of minimal changes proved impracticable, notwithstanding every effort to insure a proper attitude on the part of the observer. Thus a typical series, proceeding from too high (4.9 vib.) to objective equality, contains the following series of judgments,—higher (4),² slightly higher (2), equal (2), lower (1), equal (2), lower (2). When the variable proceeded in the reverse direction, *i. e.*, from objective equality upward, the judgments were,—lower (2), slightly lower (2), higher (6), equal (1), higher (3).

Merkel's modification of right and wrong cases met with no better success. Despite all attempts to avoid bias *M* judged 'unequal' almost invariably even with a very small *D*, being aware that differences were always being given objectively.

It was necessary, accordingly, to introduce 'equal' cases, and Kämpfe's form of the method was employed. This was more satisfactory, though an inexplicable tendency to give the judgment 'higher' modified the results of some series. In 200 cases, with $D=0.7$ vib., *M* had 65.5% right, 24.5% wrong, 9.5% equal and 0.5% doubtful cases. As computed from Fechner's table, these figures give $S=0.18$ vib., a value slightly, but inappreciably, less than the average difference limen for practiced observers in this region. When we take into account the relatively small number of cases used, and the existence of an obscuring judgment tendency, it is safe to con-

¹The figures in parenthesis indicate the number of times the judgment was given successively.

clude that *M's sensible discrimination is no finer than that of musical observers who have no absolute pitch memory*. The absence of any beneficial effect from the absolute pitch memory is attested further by the fact that no absolute memory of the stimuli employed was developed although the tests succeeded each other quite rapidly.

Summary. This study has analyzed the basis and conditions of absolute pitch memory in the case of a single observer. The following facts have been ascertained.

(1) This particular pitch memory appeared rather spontaneously in a fairly developed form at about the age of 12 years, and is probably the outcome of an inherited musical *Anlage* supplemented by early musical training.

(2) Judgments of pitch can be made of either instrumental or vocal music, though the latter is more difficult.

(3) In all cases the judgment is direct, very rapid and in terms of visual-motor imagery having reference to the piano key-board.

(4) Under optimal conditions piano notes can be correctly identified approximately 92 times in 100; the errors are then mostly semitones and never exceed a tone.

(5) Accuracy of recognition is diminished if the clang-color of the note is unfamiliar.

(6) The octave in which the given note lies is never mistaken.

(7) Recognition is most accurate in the once and the twice accented octaves.

(8) Contrary to the generalization of Abraham, this observer, though possessing but a fair accuracy of recognition, is able to image and reproduce assigned pitches with as much accuracy as she can name them when heard.

(9) Sensible discrimination is no finer than that of the average practiced musical observer and is not aided in any way by the absolute pitch memory.

(10) With an observer of the 'subjective' type, the gradation methods are not practicable: Kämpfe's modification of right and wrong cases seems most satisfactory.

II.

The second study was planned to investigate rather thoroughly the mental type of an unmusical observer, to examine the factors which prevented good discrimination, and finally, to see whether there could not be found in systematic drill and coaching properly directed some remedy for the defects revealed.

Miss M. B. Park, a Senior in Cornell University, was selected as observer for this study, because, while she represented the extreme type of unmusical observer, she nevertheless found marked pleasure in listening to music and was eager to improve her knowledge of it.

General 'Ideational Type.' An examination *P*'s mental imagery by the questionnaire method¹ showed that, of the minor senses, taste imagery was very poor, that of smell entirely lacking, while tactual, thermal, and organic sensations were rather better than the average. Emotional associations were, in consequence, quite vivid.

As regards the 'higher' senses, visual imagery was perhaps equal or somewhat better than the average, the visualization of the characters and scenes of a novel, for example, being so satisfactory and distinct as to cause a dislike of illustrations in the book itself. Auditory imagery, music excepted, is also at least equal to the average. Thus *P* can recognize her friends by their voices, can ideate easily the buzz of an induction coil, the crack of a whip, the hum of bees, the slam of a door, the clink of teaspoons in a saucer; but the sound of a church bell is imaged with some difficulty and the beat of rain against a window-pane can not be imaged at all.

In general *P* makes much less use of auditory than visual imagery. "A thing seen means much more to me than a thing heard." Musical imagery will be discussed below.

She easily distinguishes waltzes from two-steps, not directly as musical (auditory) rhythms, but from the way her feet naturally move in dancing to them.

P's ancestors were musical on the paternal side, unmusical on the maternal. None of her brothers or sisters is musical. During her childhood she heard little music, was taught none.

¹As given by Titchener; *Manual of Experimental Psychology*, I, 198 ff.

Her parents assumed that she was unmusical and never encouraged her to try to sing. On the contrary she was told, as a matter of fact, that she would never be able to sing. In consequence she became, in her girlhood, very timid about her lack of musical capacity. When placed in a school chorus at the age of fourteen she shrank from any attempt at singing, saying when urged, "I don't even know how to try." She was excused from the exercise because she received no benefit from it.

Of the rudiments of written music she has merely a hazy knowledge of the names of the notes and their places on the staff.

On the other hand there is to be noted an enjoyment in music, rather uncommon for the unmusical, especially the music of the violin, church organ and other instruments. Such music arouses vivid emotional reactions and often complex associations. "I make up a story about each piece I hear." There are also interesting synæsthetic associations of tactual and visual form with music. Thus the voices of singers are square, thin, round, etc.; violin notes are similarly round, square, triangular, flat, thread-like (drawn out), etc. Round notes are smooth and stand out alone; "triangular ones have corners which would hurt if they touched you." These simultaneous form associations seem to me interesting, as possibly indicating a general tendency, on the part of unmusical individuals, to transfer musical perceptions from auditory into other modalities.

Musical Imagery. The prevalence of centrally excited tunes seems to us the most curious thing in *P*'s ideational type. Tests with the piano, while actually listening to the notes, showed (1) that she could not distinguish any difference between a major and a minor triad, (2) that she could not detect changes of a half tone in a 'familiar' melody, (3) that she could not detect harmonic errors save those of a striking sort. Furthermore other tests showed that her memory for pitch and for melodic forms was extremely poor.

She could not, for example, recall, or even recognize when played, the simple sol, mi, sol, do, sol, mi, do, mi, of the library tower clock in announcing the hour, though this must have

been heard two or three thousand times during her life at the university. In fact, she never even knew that the quarter, half, three-quarter and hour bell-strokes were always uniform.

Yet in answering the questionnaire *P* reported that "there is scarcely any time when 'tunes' of some kind are not felt." She says further, "I can sing tunes 'on the inside,' but cannot imagine myself singing them 'outside.' I cannot mentally hear an organ play or people sing any assigned selection. I simply have what are tunes to me, but they may not be to other people. I don't recall music, but imagine it."

Since *P* does not sing it is difficult to check the correctness of these 'tunes.' But she whistles somewhat, and judged by this form of expression the imaged melodies are not at all accurate. Thus she said she could ideate a certain 'familiar' church hymn in a way that seemed to her entirely correct, but when she whistled it, it was difficult to recognize the tune save from the rhythm, which was correctly given.

We conclude, therefore, that the ideated tunes which form such a predominant feature of this unmusical observer's mental imagery, are, for the most part, purely imaginative and that, when they take the form of the reproduction of music previously heard, they are very inaccurate. It may be surmised, too, that the auditory element is subordinate to the rhythmical, *i. e.*, that the so-called 'tunes' are not so much tonal as 'motor.'

Experimental Tests. As an observer, *P* formed a decided contrast to *M*. She had the advantage of training in laboratory psychology and, in addition, fell naturally into the attitude of an 'objective' observer. Her answers were given carefully and with assurance. For sensible discrimination two instruments were employed,—the piano, to meet the usual musical conditions, and the Stern tone-variator, to secure finer gradations and equalized intensity.

A. With the variator. In the first crude tests *P* was frequently unable to judge correctly a D of 12 vibs. (with standard approximately 250 vibs.). But practice rapidly increased the discriminative sensitivity. In the regular series it was found more advantageous to employ Kämpfe's form of right and wrong cases than minimal changes. When a D of 2.8 vibs. was used, 200 tests gave 78% right, 9.5% wrong, 9%

equal and 3.5% doubtful cases. As computed from Fechner's fundamental table these figures indicate 0.52 vibs. as the value of S , a value obviously not much greater than that of practiced and musically trained observers.

B. With the piano. Two series by the method of right and wrong cases were conducted with the piano, the first before, the second after, the tests with the variator. Three pairs of stimuli were chosen, the notes c' and c' sharp, f' sharp and g' , and b' and c'' , D being, therefore, approximately 17, 24.5, and 33 vibs. respectively.

In the first series 40, 74 and 70% of right cases were recorded for these three pairs of notes, indicating a somewhat supraliminal difference in the two higher pairs,¹ but a subliminal difference in the lower pair. It seems unwarrantable that this curious drop should be due merely to the slightly lower tonal region and the consequent diminution of D . And, in fact, an examination of the judgments reveals the existence of a strong tendency to pass the judgment 'lower' when the pitch of the stimuli was lower than the once accented octave. I shall discuss this more fully below.

In the second series with the piano there were 68, 68 and 78% of right cases for the three pairs of stimuli respectively. It will be observed that the practice with the variator, which intervened between the two piano series, did not appreciably improve the discrimination of half-tones upon the piano. The right cases for the lower pair have, to be sure, now exceeded 50%, but the same tendency to judge 'lower' is still prominent.

Secondary Factors. It is well known that the pitch discrimination of an unmusical observer, whose mind moves with difficulty in the sphere of tone perception, is easily influenced by secondary factors which do not affect the judgment of a practiced musical observer. It was one of the objects of this study to detect the most prominent of these influences. In the case of P , the variator and piano tests above mentioned, when supplemented by special piano tests, revealed clearly as many as six conditions or influences that made against successful discrimination.

¹ It may be noted, however, that fewer right cases are recorded than for the variator where D was only 2.8 vibs.

(1) First may be mentioned the peculiar individual tendency already cited,—that of judging 'lower' when the stimuli were of relatively low pitch. In the first piano series of 50 tests this judgment occurred 40 times, in the second 39 times, when the lower pair of stimuli was used.¹ The strength of this tendency may be seen by the results of subsidiary tests scattered over various octaves of the piano, in which it was shown that 'lower' was judged almost invariably when the stimuli fell below the once accented octave, and that this was true in some tests when the stimuli were separated, not by a half-tone, but by as much as a fifth, or less often, by more than an octave.

(2) There was a marked daily rise in efficiency, apparently a sort of '*Anregung*' or 'warming up.' On this account the observations during the first 15 minutes of each experimental hour were rejected from the computations; otherwise the percentage of right cases would be very much lower. A typical series of 25 tests, the first in the hour, contains nine doubtful cases.²

(3) Although the tests were made in close succession and with a relatively large D, there were no traces of judgments in terms of absolute pitch, *i. e.*, no judgments given as soon as the first stimulus sounded. The reason for this was easily shown to lie in *P*'s inability to hold any image of the standard in memory for longer than four or five seconds, for when, accidentally in the variator tests, too long a time-interval (five or six seconds) elapsed between the stimuli, no judgment could be made. "I lost the first one before the second came."

(4) On the other hand, too brief duration of the stimuli, or too short a time-interval, caused equal difficulty.³ This was brought out clearly by subsidiary piano tests in which it was found that judgments of higher or lower could not be made at all

¹The tendency was evidently here strong enough to vitiate the method. Had the remaining ten cases received the same judgment, the D in use would have, theoretically, stood exactly at the limen, *i. e.*, 50 instead of 46% of right cases.

²Almost invariably cases in which a difference was correctly judged, but its direction quite unknown.

³In the regular variator tests the interval was kept at approximately 1.50 seconds; in the piano tests, approximately one second.

when two notes separated by *more than two octaves* were struck briefly and in quick succession (*e. g.*, as 16th notes).

(5) Throughout all the tests the tendency to confuse intensive with qualitative changes was strikingly manifest. A slight or moderate accentuation caused the accented note to be judged 'higher;' a strong accentuation produced a general confusion such that no judgment at all was possible, even when the stimuli were *more than an octave apart*. Unquestionably this is a most important source of confusion to the unmusical. In our own tests it may account for the fact that *P* gave no more right cases for D's of from 17 to 33 vib. with the piano than for a D of 2.8 vib. with the tone variator whereby accidental variations of intensity were avoided.

(6) A pitch difference that could be discriminated with tolerable regularity, became imperceptible when the clangs were given in fusion with one or more other clangs. Thus the half-tone *e'* *e''*-flat was, as a rule, correctly judged, but, when given simultaneously with *e'* and *g''*, not only was the direction of the change of the moving clang not discriminated, but no difference whatsoever was perceived between the two experiences, *i. e.*, there is for *P* no difference between a major and a minor chord. It might be predicted, possibly, that the qualitative relations of *e'* and *e''*-flat would suffer obscuration by fusion, but the degree of the obscuration seems unexpected until we remember *P*'s inability to hold a pitch in memory. Evidently this lack of accurate tonal imagery makes it impossible for *P* voluntarily to place her attention upon the constituent members of the chord, and to observe the qualitative changes.

We may summarize these tests of discrimination as follows. *We have found that a typical unmusical observer, when placed under proper conditions, may discriminate pitch differences of less than three vibrations correctly in 75% of the tests, but if the stimuli are of relatively low pitch, if they are given without any preliminary 'warming-up,' if the time-interval between them exceeds four or five seconds, if they are given too briefly or in too quick succession, if they are of unequal intensity, or if they are presented simultaneously with one or more other similar stimuli, then discrimination becomes either difficult or quite impossible,*

*and it may then remain impossible even when D is represented not by a few vibrations, but by musical intervals of one or two octaves and more.*¹

As already intimated, in the original plan of this study it was intended to throw light upon the relation between practice and constitutional tendencies. Musical inefficiency is often, like bad spelling, considered to be a fundamental, inherited defect, incurable by any amount of attentive effort. It may be readily admitted that it would be a hopeless task to attempt to transform into a skilled musician a person who is not only unmusical, but also utterly without interest or pleasure in music. But may not the case be otherwise when the unmusical individual finds pleasure in listening to music and is actively desirous of overcoming the deficiency of which he is conscious? May not insistent coaching and practice of the right sort supply to such an one at least the necessary structure, the basis upon which musical appreciation must rest?

Stumpf² has proposed as tests of musical capacity the ability (1) to sing a given note struck on the piano, (2) to judge which of two successive tones is the higher, (3) to judge whether one or two tones are present in fusions of high and low degree, and (4) to determine the relative pleasantness or unpleasantness of two chords separated by a short pause. If, then, any sort of educative influences could train an unmusical individual to meet these four tests successfully, the possibility of passing from the 'unmusical' to the 'musical' class would be open to any who would try. A new incentive, too, would be given the musical training of children, if we knew that environmental, rather than hereditary or innate, influences were responsible for the closing of one of the great avenues of æsthetic expression and enjoyment.

To the solution of this problem the present study has contributed little. Several experimental hours were given over to coaching, especially upon the pitch discrimination of piano notes of unequal intensity. The results were negative. As, moreover,

¹ Doubtless not all these conclusions would be applicable to every unmusical observer. We have summarized the results only for observer P under the conditions of our tests.

² *Tonpsychologie*, II, 157.

we have seen that the considerable practice in discrimination with the variator did not appreciably affect the discrimination of piano half-tones, it is evident that the study, as a whole, has rather strengthened the idea that an unmusical observer is constitutionally and inevitably unmusical, that there is an unmusical *Anlage* (if the term can be applied to a deficiency) quite as much as there is a musical *Anlage*, or a more specific *Anlage* to absolute pitch memory as in the case of *M*.

At the same time, the period given to the attempt to train observer *P* was extremely short. Possibly a much longer attempt would have accomplished something definite and permanent.¹ At any rate I believe that it is still an open question, and one worthy of solution, as to whether musical incapacity, especially when discovered in early childhood, may not be remedied by proper training. What is needed first, however, is a number of detailed studies of unmusical observers to supply data in regard to the type. I trust that the present contribution may interest others in the problem.

III.

MEMORY AND PITCH-DISCRIMINATION OF CHORDS AND MELODIES.

The idea of investigating the relative integrity in memory of the pitch of a chord or melody as compared with that of a clang, occurred to the writer in connection with some personal experiences which seemed to indicate that the pitch of a musical selection was correctly identified after intervals of time longer than those which, according to my experiments, limit successful recognition of isolated notes. It might be expected, too, that a chord or a melody, in virtue of complexity, would afford a less artificial material for experimentation, as the conditions would then more nearly resemble those of actual musical experience. It might be expected further, that, in accordance with the principle of individuation, a chord or melody, being more complex and specific than a single note, would, on

¹The rapid daily rise of efficiency shown in the tests lends some countenance to the general hypothesis that longer practice would bring noticeable improvement.

this account, be more easily retained in memory and more easily discriminated from other contents. Professor I. Madison Bentley, an experienced and musically gifted observer, kindly acted in that capacity in these tests.

A. THE CHORD.

Instrument and Method. At the outset the choice of an instrument presented difficulties, for the test required fine and rapid adjustment of at least three pitches.¹

In the absence of suitable instruments of precision we sought to take advantage, for some tentative tests, of the equal temperament afforded by the piano. The disadvantage of the piano, too large pitch differences, was overcome in three ways, —by the use of distraction; of a lengthened time-interval; and of pitches chosen as low as possible in order to minimize the difference in absolute vibration rate.

After several trials the following conditions were found most suitable. The triads *B-d-sharp-f-sharp*, and *c-e-g* (approximately 122, 156, 183.5, and 130.5, 163, 196 vibs. respectively) were used as stimuli. The method of right and wrong cases was followed with a time-interval of 40 seconds between the stimuli and with distraction obtained by reading during the interval. Intermingled with the triad series were two series of single notes given under like conditions. For these single stimuli the limiting notes of the triad, *i. e.*, *B-c*, and *f-sharp-g*, were chosen.

Quantitative Results. The experiment was so tedious that

¹ Thus, assume the triad *c-e-g* to be the standard. The Appunn tonometer might give this chord by the use of stops 528, 660, and 792 vibs. The smallest pitch-interval possible is four vibrations. If we take for the variable stimulus the next stops above the standards, we obtain the series 532, 664, and 796 vibs., whereas, in order to form a perfect triad upon the basis of 532 vibs., the two higher stops should give 665 and 798 vibs. respectively. The Appunn tonometer, without special tuning is, therefore, not available for the purposes of this experiment, for the mistunement of intervals which it necessitates is a disturbing factor sufficient to vitiate the tests. Tuning forks are difficult to adjust both delicately and quickly and to strike simultaneously. The newly improved Stern tone-variator is the only instrument satisfying the peculiar conditions of a test in the *sensible discrimination* of the pitch of chords.

but a small number of cases were tried, but the result was so unequivocal and was so firmly substantiated by the introspective reports as to leave no room for doubt upon the score of incompleteness. With the lower pair of single notes there were 75% right cases, with the upper pair of single notes 85%, with the triad only 45%. In other words, *under the conditions of the experiment a half tone is a sub-liminal difference for a triad, but a supra-liminal difference for single clangs in the same region.*

Qualitative Results. The introspective reports show that the distraction (reading difficult technical papers in which the observer was interested) was, as a rule, broken by one or two momentary resurgences of the image in a faint, cloudy form. Less often the distraction was complete. In no case was the image present in consciousness distinctly through any appreciable part of the forty seconds time-interval. Two seconds before the second stimulus a warning 'now' was given. At this time it was not uncommon for the image of the first stimulus to reassert itself in consciousness, either of its own insistence or as the result of active 'searching' on the part of the observer. On the other hand, in many, possibly in the majority of the tests, there could be seen that very interesting phenomenon of a correct judgment passed with assurance in the utter lack of anything approaching an image of the standard stimulus. Many times when *B* was aroused from his reading by the 'now,' running commentaries on the judgment process like the following were noted. "Is that the second note? . . . It is! I have no idea where the first was. . . . Well, then, this must be 'higher.' It feels that way, and I am quite sure about it."

Wherever the mechanism of the decision was revealed it was evident that the isolated clangs were judged more obviously upon a pitch basis,—not wholly auditory, but rather upon the basis of a quasi-spatial one-dimensional continuum with vague visual references to their positions upon the piano key-board. The triads, on the other hand, did not differ from each other so clearly in pitch or in their spatial position in the continuum. Generally the difference between the two triads was easily noted, but the direction of the difference was uncertain. When the direction of the difference was judged it was commonly upon

the basis of "a kind of exciting brightness" if 'higher,' and upon the basis of "a kind of subdued gentleness" if 'lower,' rather than upon any semi-spatial 'upness' or 'downness.' A similar distinction between the clang and the chord is remarked in the imaging of the first stimulus,—as, for instance, just after the second warning signal; in such cases the chord was imaged as a whole with little spatial reference, though by putting attention upon a note in the chord, the 'place-pitch' of this note could be ideated.

In the light of this introspective evidence and of the striking numerical result, we conclude, therefore, that *the pitch of a chord is more difficult to remember and to discriminate than the pitch of a single note.* A chord is, to be sure, more specific, more individual, than a clang, but its individuality as a chord, as a specific musical complex, is gained at the loss of individuality of pitch. As a given combination of intervals a chord is specific; as an auditory quality it is less individual than a clang. The essential feature of a clang is its position in the tonal continuum. A triad has no such definite position; or rather it is three positions given simultaneously and in fusion. Thus the discrimination of its pitch is more difficult because the quality of the stimulus is less 'clean cut.'

B. THE MELODY.

The study of the memory and pitch discrimination of a melody as compared with a clang was carried on under conditions practically identical with those just described. It will be seen that the simple melodic form employed embraced all the clangs in the region occupied by the triad used above, while the highest and lowest notes (those used as single stimuli for comparison) were struck twice each and formed the accentuated notes of the melody. The melody figured, when repeated, a half tone lower, formed the other stimulus.



Results. Our experiments were too few in number to admit of definite quantitative formulation. They showed clearly,

however, that, under the conditions employed, *the memory and discrimination of the pitch of melodic form is as good as, possibly better than, the memory and discrimination of the pitch of single clangs in the same tonal region.*¹

The introspection showed that, as in the case of the triad, the image was apt to be reconstructed as a whole at the warning signal, *i. e.*, the entire melody was heard. But again, as before, there were numerous judgments based entirely upon the second stimulus.

It is evident that the use of a number of successive clangs in place of a single clang does not have any confusing effect like the use of a number of simultaneous clangs. On the contrary, the effect is probably to multiply the opportunities for identifying the pitch of the standard stimulus. Now, if we assume that this greater ease of discrimination in the case of the melody, which the numerical results indicate, is a fact, we may further ask whether the use of a melodic form *as such* facilitates the discrimination. In other words, would not the same effect have been secured by striking a single note seven times instead of striking five different notes once or twice each? I am inclined to answer in the affirmative. The essential feature of a melody lies not in its pitch, but in the musical relationships of its constituent members. We should, therefore, *a priori*, suppose that the introduction of a melodic *form* into a series of successive clangs would not *per se* facilitate either the memory or the discrimination of the absolute pitch of the clangs involved.

Unfortunately the limitations of the time available for this test have neither permitted us definitely to establish the supremacy of the melody over the clang nor to investigate the question just raised as to the comparative influence of the melodic form itself and that of simple repetition of a single clang.

Summary. As tested on the piano with half-tone intervals in the small octave, with a time-interval of 40 seconds and

¹The actual percentage of right cases for the melody was 95, as compared with 75 and 85 for the lower and upper limiting clangs respectively.

with distraction, the pitch of a chord is more difficult to remember and to discriminate, than the pitch of a single clang.

As tested under the same conditions, the pitch of a simple melody is as easily remembered and discriminated as the pitch of a single clang, possibly more easily. In the latter case it is not clear whether the increased facility is due to the melodic form itself or merely to the greater number of stimuli employed.

STATISTICS OF AMERICAN PSYCHOLOGISTS.

By Professor J. MCKEEN CATTELL, Columbia University.

To have the privilege of joining with other former students of President Hall in this volume, I must detach from an unfinished study of American men of science some statistics regarding our psychologists.

I have discussed in various papers the importance to psychology of the study of individual differences and have given some results of a research in which I am taking as the material one thousand students of Columbia College,¹ one thousand eminent men of history,² and one thousand American men of science.³ The thousand men of science have been selected from some 4,000 included in a biographical dictionary that I am compiling, who, in turn, were selected from a list containing some 10,000 names.

On the lists are the names of 313 who appear to me to have contributed to the advancement of psychology. Among them are many whose work is unimportant, and several who have scarcely accomplished anything beyond teaching or the writing of a text-book. There are not included, however, those who have printed a minor study and have not again been heard from, nor most of those working in other sciences whose results may be valuable for psychology, but belong elsewhere. Excluding from the group nearly all those whose work is not primarily in psychology, or in psychology combined with philosophy or education, there remain 270. Omitting those

¹ Physical and Mental Measurements of the Students of Columbia University (with Dr. Livingston Farrand). *Psychol. Rev.*, 3: 618-648, 1896. Cf. also 'The Correlation of Mental and Physical Tests.' C. Wissler. Monograph Supplement to the *Psychol. Rev.*, No. 16, 1900.

² *The Popular Science Monthly*, 57: 359-377, February, 1903.

³ *Science*, N. S., 18: 561-570, April 10, 1903. Towards the cost of computation in connection with this research, I have received a grant of two hundred dollars from the Esther Herrman Research Fund of the Scientific Alliance of New York.

whose work seems to be of the least value, and those whose addresses I have not been able to find or from whom I have no returns, I have taken a group of 200 for consideration. The first 150 or so have been selected objectively in the manner described below, the last fifty are practically those that were left. For certain purposes four groups of fifty each have been used, the first group consisting of those whose work is supposed to have the greatest merit, the second group ranking next, etc.

I have distributed scientific men among twelve principal sciences—mathematics, physics, chemistry, astronomy, geology, botany, zoölogy, physiology, anatomy, pathology, anthropology, and psychology. As there are about 200 psychologists among some 4,000 students of science, the psychologists are about one-twentieth of the scientific workers of the country. They are about equal in number to the astronomers; they are more numerous than the physiologists, anatomists, or anthropologists, and are fewer than the workers in the other principal sciences.

In each of these sciences I have asked ten leading representatives to arrange the students in that science in the order of merit. I did this (1) to select for special study the thousand regarded as the most meritorious; (2) to be able to discuss distribution in relation to merit, and to correlate merit with various qualities; (3) to learn the meaning and validity of such judgments, and (4) to have on record the order with a view to reference or possible publication ten or twenty years hence.

The memorandum sent to those who were asked to make the arrangement was as follows:

MEMORANDUM.

The undersigned is making a study of American men of science. The first problem to be considered is the distribution of scientific men among the sciences and in different regions, institutions, etc., including the relative rank of this country as compared with other countries in the different sciences, the relative strength of different universities, etc. It is intended that the study shall be continued beyond the facts of distribution to what may be called the natural history of scientific men.

For these purposes a list of scientific men in each science, arranged approximately in the order of merit, is needed. This can best be se-

cured if those who are most competent to form an opinion will independently make the arrangement. The average of such arrangements will give the most valid order, and the degree of validity will be indicated by the variation or probable error of position for each individual.

It is obvious that such an order can be only approximate, and for the objects in view an approximation is all that is needed. The judgments are possible, because they are as a matter of fact made in elections to a society of limited membership, in filling chairs at a university, etc. By merit is understood contributions to the advancement of science, primarily by research, but teaching, administration, editing, the compilation of text-books, etc., should be considered. The different factors that make a man efficient in advancing science must be roughly balanced. An effort may be made later to disentangle these factors.

In ranking a man in a given science his contributions to that science only should be considered. Thus, an eminent astronomer may also be a mathematician, but in ranking him as a mathematician only his contributions to mathematics should be regarded. In such a case, however, mathematics should be given its widest interpretation. It is more difficult to arrange the order when the work cannot readily be compared, as, for example, systematic zoölogy and morphology, but, as already stated, it is only expected that the arrangement shall be approximate. The men should be ranked for work actually accomplished, — that is, a man of sixty and a man of forty, having done about the same amount of work, should come near together, though the man of forty has more promise. It may be possible later to calculate a man's value with allowance for age.

In case there is noted the omission of any scientific man from the list who should probably have a place in the first three quarters, a slip may be added in the proper place with his name and address. In case there are names on the list regarding which nothing is known, the slips should be placed together at the end. The slips, as arranged in order, should be tied together and returned to the undersigned.

It is not intended that the lists shall be published, at all events not within ten years. No individual list will be published. They will be destroyed when the averages have been calculated, and the arrangements will be regarded as strictly confidential.

The attitude of those asked to make the arrangement was itself a matter of some slight psychological interest. Most of those asked consented, but for one reason or another a considerable number failed. For example, the ten psychologists whom I selected proved to be the ten at the head of the list. All but one of these undertook to make the arrangement; but one gave it up on account of the difficulty, one arranged the

names in groups instead of serially, and two delayed the matter. For these substitutes were secured. Many of those who made the arrangement stated that they had but little confidence in its validity; but the results of combining the ten series show that the judgments do have a certain validity, which is itself measurable.

TABLE I.

Order, positions and probable errors assigned to the first fifty American psychologists.

ORDER.	POSITIONS.	P. E.	p. e.	ORDERS.		
				II.	III.	IV.
1	1.0	0.0	0.0	1	1	1
2	3.7	0.5	0.6	2	2	2
3	4.0	0.5	0.4	3	3	3
4	4.4	0.6	0.6	4	4	4
5	7.5	1.0	0.9	5	5	6
6	7.5	1.2	1.3	6	8	5
7	7.6	0.4	0.3	9	9	9
8	9.2	1.5	1.2	7	6	7
9	9.6	0.6	0.7	10	10	10
10	11.6	3.0	2.3	8	7	8
11	12.3	1.0	1.1	11	11	11
12	16.8	1.7	1.4	12	12	13
13	17.1	1.6	1.6	14	13	16
14	17.9	2.1	2.3	13	14	12
15	18.7	1.4	1.5	15	15	14
16	19.3	1.3	1.4	19	17	17
17	19.6	1.6	1.6	18	18	15
18	21.6	2.7	2.4	16	16	19
19	21.8	1.2	1.3	20	21	18
20	22.4	1.6	1.3	21	20	20
21	24.5	3.4	3.4	17	19	21
22	27.0	2.0	2.0	23	25	22
23	29.5	4.8	3.9	22	22	23
24	37.5	2.4	2.5	26	24	26
25	37.7	5.4	5.0	24	23	24
26	40.4	6.5	7.3	27	26	27
27	41.6	6.2	7.0	25	27	25
28	42.9	4.1	4.2	30	28	32
29	44.7	4.1	4.2	29	29	31
30	44.9	4.1	4.6	34	30	38
31	45.5	4.5	4.4	31	31	35
32	46.4	4.8	4.9	32	32	33
33	47.1	5.2	5.7	37	33	30
34	48.0	5.5	5.5	28	34	28
35	49.3	6.6	7.4	35	35	29
36	49.6	4.9	4.8	38	36	39
37	49.9	5.9	6.0	33	37	34
38	51.1	5.3	5.3	40	38	40

ORDER	POSITIONS.	P. E.	p. e.	ORDERS		
				II.	III.	IV.
39	52.6	5.7	6.1	39	39	37
40	53.3	5.7	5.8	36	40	41
41	54.5	4.1	4.6	41	41	42
42	54.5	5.6	6.9	52	42	45
43	56.2	5.3	5.9	42	43	53
44	56.5	6.6	8.0	43	44	43
45	58.6	4.6	5.1	45	45	48
46	59.0	5.0	4.6	46	46	46
47	59.0	7.1	8.7	51	47	51
48	59.2	5.7	6.9	44	48	36
49	59.2	6.2	7.1	47	49	44
50	59.6	3.7	4.0	49	50	49

The table gives the results for the fifty psychologists esteemed the most meritorious. The first column, printed in heavy type, gives the serial order in which they stand, the second column the positions as deduced from the average of the ten arrangements, and the third column the probable errors of these positions. The four remaining columns give the probable error obtained by the easiest method, and the order when the figures are compiled in accordance with the different methods described below. It should be distinctly noted that these figures give only what they profess to give, namely, the resultant opinion of ten competent judges. They show the reputation of the men among experts, but not necessarily their ability or performance. Constant errors, such as may arise from a man's being better or less known than he deserves, are not eliminated. There is, however, no other criterion of a man's work than the estimation in which it is held by those most competent to judge. The posthumous reputation of a great man may be more correct than contemporary opinion, but very few of those on this list of psychologists will be given posthumous consideration. I am somewhat skeptical as to merit not represented by performance, or as to performance unrecognized by the best contemporary judgment. There are doubtless individual exceptions, but, by and large, men do what they are able to do and find their proper level in the estimation of their colleagues.

It will be noticed that the probable errors tend to increase from the top of the list downward. These errors show the validity of position and also the amount of difference between the men. No. 3, with a grade of 4 and a probable error of

0.5, has about one chance in three of being higher than the grade of No. 2, or of being lower than that of No. 4. The chances are more than 10,000 to one that he is not as high as No. 1,¹ or as low as No. 5. At the bottom of the list the probable errors show that the positions are probably correct within about ten places. The chances are about one in four that No. 40 does not belong among the first fifty psychologists.

The men at the top of the list differ much more from each other than the men at the bottom. The average probable errors of the first ten is less than one, and they cover a range of about ten points; the average probable error of the last ten is about six, and they cover a range of about five points. If we may assume that the differences between the men are directly as the difference in grade and inversely as the probable error, there is about twelve times as much difference between the individuals near the top of the list as between those near the bottom. It is as easy for a man towards the bottom to gain twelve places in the estimation of his colleagues, as for a man near the top to gain one place. There is about as much difference between No. 1 and No. 2, as between No. 2 and No. 8, which again is about equal to the difference between No. 24 and No. 50.²

The table indicates that the differences are not continuous, but that there is a tendency towards the formation of groups or species. This may be due to accidental breaks in the series, but similar breaks in approximately the same places occur in other sciences. Among the psychologists, and in several other sciences, there is one man who stands distinctly at the head, completely separated from those next to him. Should No. 1 die, his place would not be taken by No. 2, but Nos. 2, 3, and 4, and others would each be raised a little. This man is a great genius, who has an international, and will have a posthumous, reputation. Then, among the psychologists we have

¹ No. 1, having a probable error of 0, has his position determined in the negative direction only by the positions and probable errors of Nos. 2, 3 and 4.

² I am quite willing that these statements shall be regarded as rough approximations. There are certain theoretical questions involved which I hope to discuss on a subsequent occasion.

Journal of H. S.

a distinctly marked group of three; they are men who doubtless have a reputation beyond their own science and their own country. There is next a group of seven, so well marked that it is very unlikely that any one of them belongs above or below this group. There is next an equally distinctly marked group of twelve. The break at the end of this group is eight whole points, and would be fifteen points did not the individual Nos. 21, 22, and 23 partly bridge it. Nos. 21 and 23 have comparatively large probable errors, and in my opinion belong to the upper group. No. 22 appears to me to belong to the upper group by ability, and to the lower group by performance, and will probably join later one or the other group. There is, consequently, a well-marked break separating the first 20 or 23 psychologists from those below. The chances are very great¹ against Nos. 19 or 20, being as low as the grade of No. 24. The break is also shown in the probable errors, which increase almost suddenly from about 2 to about 6. The average age of the group from No. 24 to No. 50, is 39.93 years, and of the group from No. 12 to No. 23, is 40 years. It appears to me that not more than three or four of those in the lower group will ever pass over to the upper group. There is about the same percentage further down in the second and third fifties who are nearly as likely to enter the first group. We thus seem to have two main groups or types; the leaders, and the men of moderate attainments, the leaders being about one-tenth of the whole number. The leaders are again broken into four groups—say, of great genius, of moderate genius, of considerable talent, and of talent.² It appears that analogous groups are found in other sciences, and it is probable that they also exist in other lines of intellectual activity and performance.

The lower group, from 24 to 50, and on down to the end of the 200, shows no further break and no very great increase in

¹ More than a million to one. It seems fair to emphasize the accuracy of this determination, showing that psychology even in cases as complicated as this can rival in exactness the physical sciences.

² The two lower groups are not, however, uniform; they include two or three men who would rank higher for their contributions to philosophy, and two or three who are young and will almost certainly advance to a higher group.

mediocrity. The group is heterogeneous and may be divided into three main subgroups — psychologists of moderate ability who have found their level; young men who may improve their positions, even going to the top of the list; and students of other subjects, almost exclusively philosophy or education, who have made, or are supposed to have made, contributions to psychology.

In the average positions given in the table, the ten arrangements are included without omissions. Those who made the arrangements do not, however, possess equal information or judgment, and here again it is possible to measure traits that at first sight appear to be scarcely comparable. The table gives the average variation of the judgments of each observer from the average of the judgments of the ten observers. Data are given for groups of ten (I, the ten psychologists highest on Table I, etc.), which show a considerable degree of constancy.

TABLE II.

Measurements of the accuracy of judgment of ten observers.

	A	B	C	D	E	F	G	H	I	J	Av.
I.	1.6	5.2	2.3	3.1	1.9	2.8	1.8	2.4	5.0	2.6	2.87
II.	4.9	7.0	8.7	7.8	6.3	4.2	10.2	6.1	7.0	5.1	6.73
III.	7.1	11.1	13.5	12.4	24.2	16.2	12.5	12.0	16.9	27.6	15.35
IV.	13.8	18.0	16.7	18.4	18.8	18.1	22.7	25.4	21.9	25.5	19.93
V.	12.1	17.4	21.1	21.1	13.2	26.6	21.7	24.7	22.9	25.5	20.63
Av.	7.9	11.74	12.46	12.56	12.88	13.58	13.78	14.12	14.74	17.26	13.1
V.	-5.20	-1.36	-0.64	-0.54	-0.25	+0.48	+0.68	+1.02	+1.64	+4.16	±1.60

The observer A is always more accurate than any other observer, except in one case in the fifty. The validity of judgment of the ten observers varies from 7.9 to 17.26, or about as 1:2, which is approximately the variability that I have found in normal individuals in other mental traits, accuracy of perception, time of mental processes, memory, etc. The departures from the mean reliability of judgment, given in the last line of the table, indicate that accuracy of judgment tends in a general way to follow the normal distribution of the probability curve. As the validity of the judgments vary to a measured degree, the arrangements made by the individuals should be weighted. I have not undertaken the somewhat

tedious calculations necessary; they would not considerably alter the order, but would make it somewhat more exact, at the same time decreasing the probable errors.

Taking the individual departures from the mean of the ten estimates in each case (the residuals or errors), and reducing them to a common standard, we secure the distribution of the 500 'errors' shown in the figure. The curve is fairly regular,

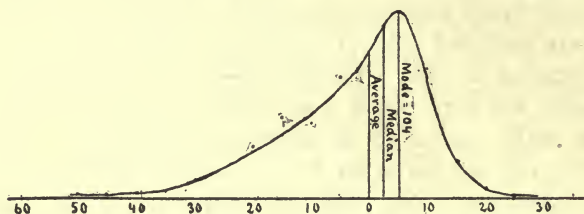


Fig. 1. Distribution of 'errors' of judgment.

but asymmetrical. The judgments do not follow the distribution of the probability curve and do not represent the typical case of errors due to a large number of independent and small causes equally likely to be positive or negative. The average judgment would give a man a lower grade than the median judgment, and this again is lower than the mode or most common judgment. This means that in judging the position of a man, the individual judgments are more likely to depart considerably from the average in placing him too low than too high. This is especially the case for men near the top of the list. The judgments do not, however, depart abnormally from the mean. According to Chauvenet's criterion, there should not be in 500 observations any over about five times the probable error. There are in this series but three departures larger than this, which all occur near the top of the list, and in the negative direction. The regular character of the curve seems to show that the extreme judgments are not abnormal. There is no evident group or species, due to ignorance, prejudice or the like, such as might conceivably have existed.

Under these circumstances, we may ask, what is the most probable position of the individuals? Should we take the average, median or mode, or possibly some compromise, such as would be obtained by omitting certain of the more divergent

judgments? It seems to me that any of these procedures would be correct, but that the meaning would be different in each case. The average is what it professes to be — the average judgment; here a single unfavorable judgment is more likely to be large than a favorable one, and a single judgment may pull a man down further than it can lift him up. The median is also what it professes to be — the middle judgment, omitting the divergent judgments and not taking into account the size of the departures. The mode, or most common judgment, comes out clearly as the result of 500 measures, but would not in the case of the ten judgments for each individual. It has seemed to me that in many psychological measurements, there is something to be said for a compromise between the average and the median.¹ The curve of distribution is usually asymmetrical, and the more divergent measures may be due to abnormal conditions. Experimenters have been led to reject observations that appeared to be abnormal, and have sometimes done so in a way that has invalidated their results. I can see no theoretical objection to a criterion such as Chauvenet's, or to the plan used by me of rejecting in each series a certain fixed number of the more divergent results, the average of the entire and of the corrected series being given. It is an advantage to do away with incorrect or even abnormal values in an objective manner, and so far as normal values are eliminated, we are simply adopting a compromise between the mean and the median.

In the case before us, the regular character of the curve of distribution and the fact that the departures are scarcely larger than the theory of probabilities would lead us to expect in the case of an orthodox distribution, show that the more divergent judgments are not abnormal. The most divergent errors were in the cases of Nos. 10 and 21, and it appears to me that there is some slight chance that each of these extreme judgments is correct — a larger chance, indeed, than the theory of probability indicates. On the other hand, the incidence of these extreme judgments on the individual seems unfair. If we had a hundred judgments of each individual, we might ex-

¹ Psychometrische Untersuchungen. Philos. Stud. 8: 316 f., 1886.

pect one so divergent, but its effect would be nearly eliminated in the average. When a hundred judgments are given to ten individuals, we may expect that there is some slight chance that one of these individuals should be considerably lower than his grade indicates; but the incidence on a particular individual seems to be a matter of chance. Thus, No. 10 is by a single divergent judgment carried down two places and given a probable error three times as large as he would otherwise have. The use of the median or the omission of the more divergent judgments by Chauvenet's criterion, or otherwise, tends to give most of the individuals a higher grade, but scarcely alters the position of any one beyond the limits already indicated by the probable error.¹

I have used in my discussion the simple averages from all the judgments. In the table, however, is given the order of the individuals when the two highest and the two lowest judgments are discarded (Order II), when Chauvenet's criterion is approximately applied² (Order III), and when the median is taken (Order IV).

A careful consideration of the individual figures leads me to believe that the use of all the judgments is quite satisfactory, the exceptional judgments not altering the positions to a considerable extent, or beyond the limits of the probable errors except in one case. I am inclined to believe, however, that the most valid order is that in which the six most accordant judgments were used, and the order next in validity is that in which Chauvenet's criterion is applied.³ The median seems

¹One disadvantage in the use of the median or in the adoption of any criterion rejecting the more divergent observations, is that it becomes difficult or impossible to assign a probable error. Attention should be called to the fact that the probable errors deduced by the ordinary formulas, and given in the table, do not hold for a curve of distribution such as we have here. They must be regarded as approximations only.

²That is, variations are omitted that are more than about three times the probable error, there being ten estimations in each series.

³The difficulty with Chauvenet's criterion is that the probable error is not very exactly determined in a series of ten judgments. Thus, if two divergent judgments in the same direction occur in a series of ten, the probable error is so enlarged that the divergent judgments are not eliminated, whereas they would be eliminated if we used the average probable error of the adjacent individuals.

to be the least satisfactory determination, as, owing to the comparatively small number of observations, the position is determined by chance.

There are given two probable errors. The former (P. E.), has been deduced by way of the squares of the residuals, the latter (p. e.) by multiplying the mean variation by 0.28.¹ There is really but little difference in the results, and in view of the character of the curve of distribution, I doubt whether this is significant. It seems to me to be a waste of time to use the method of least squares in such cases.

The curve of distribution of the men on the list approaches roughly the distribution of the probability curve at the positive end. We have, as a matter of fact, only fifty psychologists on the table given above, and if we assume that all the psychologists of the country form a 'species,' we have only the upper fourth or fifth of the distribution. On the average about one hundred psychologists were arranged in the order of merit by the ten observers, but the number varied from 53 to 175. I have counted out the order and the probable errors up to 120 places, but after about 75 places they have not much validity. When an individual was not included on one of the lists, he was given a grade about midway among those omitted; but this procedure is inexact and tends to decrease unduly the probable errors. It would be of interest to secure the complete curve of distribution, but it is scarcely possible to do this, as the men in the lower half are not known to those who make the arrangements. In this case the sizes of the probable errors depend on the ignorance of the observer as well as on the similarity in merit of the men. I am myself fairly well acquainted with the work of our minor psychologists. When I undertake to rearrange the men, there is but little variation in the position of those near the top of the list, while the variations increase constantly as I go downward. Ten arrangements made by the same individual, would give an order and probable errors analogous to those of the table, but this is an experiment that cannot be made accurately, as the observer remem-

¹ *I. e.* $\frac{\text{Av. E.} \times 0.845}{\sqrt{n-1}}$

bers his previous arrangements, thus introducing a large constant error. I, however, regard my own judgment as subject to a probable error somewhat similar in size to that given in the table as deduced from ten observations. This subjective probable error increases continuously as I go down the list, and I assume that there is no descending half on the negative side of the curve of distribution. Starting from the median man there is a rough approximation to the probability curve on the positive side, but on the negative side the curve continues to rise, but with increasing slowness. This refers to the great majority of those on the negative side of the median, their work being simply mediocre or unimportant. There are, however, a few individuals in each science whose work is inaccurate or bad. They belong to a different 'species,' and are scarcely comparable with the others. There would be some agreement as to their order, and they would form a descending curve. They are, however, far too few to balance those above the median. It should be noted that while for men whose work is well known the order and probable errors nearly coincide with their performance and ability, in the case of men who are imperfectly known, the probable errors are a function of this ignorance. If the men below the median were as well known as those above, the probable errors might not continue to increase as we go down. Among my students those weak in psychology do, in a general way, seem to balance those who are good, in accordance with the distribution of the probability curve. But the students on the minus side do not become professional psychologists. However, even if we assume that all the people of the country are psychologists of a sort, their distribution seems to be represented more nearly by the positive half of the probability curve than by the whole curve.

It is claimed by Mr. Galton and others that the probability curve represents the normal distribution of ability, the idiots balancing the geniuses, etc. But I am not sure that this is correct. For example, it seems to me that in poetic ability the great poets differ fully as much from the poor poets, as these do from the mass of mankind, and that the latter do not differ so much among themselves in poetic ability as do poets. Poetic ability would thus be distributed as suggested for psycho-

logical ability. Ability and performance may not be due to a large number of small causes equally likely to be positive or negative, but there may be a certain standard ability which may be increased by a few relatively large causes, hereditary and environmental, but not correspondingly decreased.

It is my intention to make a somewhat elaborate study of the natural history of American men of science. I am at present, however, only able to give in regard to the psychologists some statistics of their academic origin, course and destination. The accompanying table contains data regarding the education of the two hundred psychologists. There are given the numbers that took the A.B. or equivalent degree from various institutions, the numbers that pursued graduate studies without taking the doctor's degree at the given institution, and the numbers that were given the doctor's degree. Institutions are included that were attended by three or more students. Most of those who took the bachelor's degree pursued graduate studies, and they may be entered in the three different divisions. Those who took the doctor's degree were also graduate students at the institution granting it, but they are not entered under this heading in the table. Many pursued graduate studies at two or more universities and are included under each. There are consequently duplications in the second division, but not in the first or third. When a man has taken a bachelor's degree from more than one institution, he is credited to the first, and if the work at the second was specialized he is credited as a graduate student. The spirit rather than the letter of the classification has thus been followed. Each division of the table is divided into four columns, numbered I, II, III, IV, under which are the figures for the four groups into which the men have been classed, the fifty selected as the best being in the column under I, etc.

The most interesting fact shown by the tabulation appears to be the very large number of institutions—seventy-six—from which the 200 psychologists have come. The origin of the men is in general independent of the college at which they were educated, as is also their rank among psychologists. Harvard, with its strong department of philosophy and psychology, has not produced three times as many psychologists as

TABLE III.

The numbers of American Psychologists who have studied at different Institutions.

	A. B.					GD. STUD.					PH. D.					G. T.
	I.	II.	III.	IV.	T.	I.	II.	III.	IV.	T.	I.	II.	III.	IV.	T.	
Columbia,	2	I	4	3	10	2	I	2	4	9	3	5	9	6	23	42
Harvard,	2	6	2	I	11	7	5	4	0	16	4	4	5	2	15	42
Clark,						2	3	4	4	13	4	5	5	4	18	31
Cornell,	0	0	0	2	2	I	I	0	I	3	3	4	10	5	22	27
Yale,	I	2	I	3	7	I	2	2	0	5	2	3	2	4	11	23
Princeton,	3	6	2	4	15						I	3	0	I	5	20
Penna.,	3	3	0	2	8	0	I	2	I	4	I	2	I	I	5	17
Hopkins,						2	I	I	I	5	7	I	0	0	8	13
Amherst,	4	I	I	3	9											9
California,	3	0	3	0	6						0	0	I	0	1	7
Indiana,	2	2	0	3	7											7
Brown,	0	I	4	0	5						0	0	I	0	1	6
Chicago,	0	0	2	0	2						0	I	2	I	4	6
Michigan,	I	I	0	I	3	0	0	I	0	1	0	0	0	I	1	5
Nebraska,	2	I	0	2	5											5
Rochester,	I	2	0	I	4											4
Vermont,	2	0	2	0	4											4
Wesleyan,	2	I	I	0	4											4
Williams,	2	I	I	0	4											4
Wisconsin,	0	0	I	2	3						0	0	0	I	1	4
Lafayette,	I	I	I	0	3											3
Stanford,	0	I	2	0	3											3
Berlin,						13	10	6	3	32	I	I	0	I	3	35
Leipzig,						7	5	4	2	18	9	4	2	2	17	35
Heidelberg,						4	5	I	0	10	0	I	I	0	2	12
Göttingen,						3	2	I	I	7						7
Freiburg,						2	0	I	I	4	I	I	0	I	3	7
Bonn,						I	2	0	0	3	0	0	I	I	2	5
Jena,						I	I	I	0	3	0	0	I	I	2	5
Strasbourg,						0	0	3	I	4	0	0	I	0	1	5
Zurich,						I	I	0	0	2	0	0	I	0	1	3
Paris,						5	7	2	0	14						14
Cambridge,	0	0	0	I	1	2	I	0	I	4						5
Total.	31	30	27	28	116	54	48	35	20	157	36	35	43	32	146	419

Rochester, Vermont, Wesleyan, or Williams, though it has given its Bachelor's degree to far more than three times as many students. Yale, where philosophy and psychology have been required studies under able professors, has not produced twice as many psychologists as each of the colleges mentioned. Cornell has only produced half as many. Of the eleven psychologists from Harvard, only two are in the first fifty, and of the seven from Yale, only one. Some institutions have produced more than their share of psychologists, as Indiana and

California, but probably not more than the theory of chance distribution would lead us to expect, except in the cases of Princeton and Amherst. At these two colleges there appears to be an influence strong enough to direct men to psychology.

There is, perhaps, no other information so desirable for applied psychology and sociology as knowledge of the extent to which the performance in life of the individual is due to what Mr. Galton has called nature and nurture, respectively. Our present knowledge is scanty and ambiguous, so that it is possible to hold extreme views without coming into conflict with facts. It may plausibly be argued that a man's career is chiefly prescribed by his equipment at birth, or chiefly by his environment, education, and opportunity. It appears to me that certain aptitudes, as for music, mathematics, etc., are chiefly innate, and that kinds of character and degrees of ability are largely innate. But I have been inclined to believe that the direction of performance was largely determined by circumstance, that a man who is a psychologist might, by a comparatively slight and accidental alteration in conditions, have become a zoölogist, or a lawyer, or a man of business, and have been almost equally successful in any career. This view seems to me, however, to be traversed by these statistics, which, as far as they go, indicate that psychologists are born, not made.

After the men have graduated from college, and when their work has been chosen, they are gathered for their special studies into a few universities. It does not seem, however, that they are turned into psychologists at these universities. They simply select for study the universities that have reputation and facilities, being often attracted by fellowships or the hope that the university will assist them in securing positions. The direction of the work is sometimes determined or influenced by the university, the merit of the work but slightly, if at all. Harvard, Clark, Columbia, Cornell, and Yale have contributed to the first group 4, 4, 3, 3, 2 doctors, respectively, and to the second group, 4, 5, 5, 4, 3. There are relatively more men in the third and fourth groups from Columbia and Cornell, which is in part due to the fact that Columbia has conferred the degree frequently in education, and Cornell in philosophy, and

the men being ranked for their contributions to psychology come in the lower hundred. It should also be stated that my returns for Columbia are probably more complete than for the other universities. This would not affect the first hundred, but the total influence of Harvard shown by my statistics to be equal to that of Columbia, is probably somewhat greater. Harvard, Clark, and Columbia have had, according to the table, almost exactly the same number of graduate students; if, however, we add together the students under President Hall at the Johns Hopkins and Clark Universities, they surpass in numbers and in rank those of any other institution.

It will be noted that our psychologists have studied in Germany in large numbers, no less than thirty-five, both at Leipzig and at Berlin. Leipzig has exerted much the greater influence, having conferred seventeen doctorates on men who studied there from one to five years. Men record themselves as having pursued graduate studies at German universities when the time was a semester or less, whereas here it is practically always at least one year. It is noticeable that more men of the first group have studied and taken their degrees in Germany, and that the numbers decrease as the groups become lower. I should not interpret this as meaning that men are made better psychologists by going to Germany. Men of greater ability and enterprise are perhaps more likely to go to Germany, but the facts of the table are largely due to the circumstance that the older men have studied in Germany in much larger numbers than those who are younger.

The two hundred psychologists come originally from 76 institutions, they gather together for graduate study at a comparatively small number of universities, and are again widely dispersed. Nearly all the men are engaged in teaching, being distributed among 57 colleges and universities, 14 normal schools, and six other schools. There is, indeed, not a single person on the list who has not been engaged in teaching, though three or four have only given lecture courses incidentally. There are at present only eight who do not earn their livings by teaching or administrative educational work. I can give in quantitative terms the strength of the work in psychology in different institutions, but it may be wiser to postpone

publication of the figures until all the sciences can be treated together.

It is fortunate for psychology that there are in this country so many teaching positions to be filled. Still, the conditions present certain serious drawbacks. The time of the men is occupied in teaching, and in administrative, clerical, or missionary work, which, together with their great dispersal, is not favorable to the cultivation of a spirit of scholarship and research. There is but small opportunity for natural selection, a kind of panmixia obtaining. In other sciences larger numbers of men are needed for school work, and from these the better can be selected for the college and university positions. In most sciences there is applied work, into which those who prove inept for research can overflow.

It appears to me that psychology in America has received fewer contributions from those not professionally engaged in teaching it than is the case in other countries. In Great Britain there has always been a group of men, largely selected from the wealthy classes, who have not earned their livings by teaching, but have devoted themselves to research and authorship. Psychology in Germany has received important and numerous contributions from physiology, physics, neurology, and other sciences to an extent not approached in America.

In order to compare our productivity with that of other nations, I have counted up the first thousand references in the index to the twenty-five volumes of the *Zeitschrift für Psychologie*. These are the papers published in, or reviewed by, the journal, and are doubtless supposed to be the more important contributions to psychology. In the table are given the numbers of contributions from each nation and a classification of the subject-matter. Germany leads in productivity, though German contributors are favored in the original articles and also in the reviews of the *Zeitschrift*. This appears to be especially the case under the heading 'Physiological' (which includes pathological and physical papers). The French experimental papers include 48 by M. Binet, and if the classification had been carried to the end of the alphabet, the French contributions would have been relatively much fewer. I should infer from the table that America leads decidedly in experimental con-

TABLE IV.
Classification of Contributions to Psychology.

	Experimental.	Theoretical.	Physiological.	Total.
German,	93	99	290	482
French,	102	56	34	192
American,	III	31	11	153
English,	7	25	31	63
Italian,	9	9	39	57
Swiss,	8	7	6	21
Belgian,	8	5	4	17
Spanish,	0	0	8	8
Dutch,	2	2	0	4
Scandinavian,	1	1	1	3
Total,	344	235	424	1,000

tributions to psychology, that we are about equal to Great Britain in theoretical contributions, but are almost doubled by France and Germany, and that we are decidedly inferior to Germany, France, Great Britain, and Italy in contributions of a physiological and pathological character. The thousand contributions counted up extend into the F's or to rather more than one-fourth of the index. Thus, somewhat less than six hundred American contributions in the course of ten years have been published in, or reviewed by, the *Zeitschrift*. There were not, during this period, as many as two hundred American psychologists. But in a general way it appears that each of our psychologists has on the average made a contribution of some importance only once in two or three years.

A STUDY ON THE CONDUCTIVITY OF THE NERVOUS SYSTEM.¹

By Professor YUJIRO MOTORA, University of Tokyo, Japan.

Recently the anatomy and the histology of the nervous system have been much studied, while its physiology is as yet very little known. We are specially ignorant of the nature of nervous conduction. Many years ago certain scientists tried to identify it with electricity, but after some experimental study it was found out that there were three points of disagreement between the two, viz.: (1). The velocity of nervous conduction was much slower than that of an electric current; (2). The nerve fibers were not insulated as an electric wire is; and (3). The nervous conduction did not produce an induction current as an electric current does. These scientists, therefore, came to the conclusion that nervous conduction was a result of some chemical change peculiar to nerve fibers. They were forced to do so for the reason that they could not think of any other explanation. Our desire for scientific knowledge will not be satisfied until the details of the chemical changes accompanying the nervous conduction have been made plain. The physiology of nervous conduction is in its present state very imperfect. It is, however, impossible for us to study psychology without some knowledge of the physiology of the nerves. Such was the motive

¹ The following investigations have been pursued under the great disadvantage of having at my disposal only a limited number of books and magazines available for reference. It must, however, be gratefully acknowledged that several of the professors in the University and some others have generously furnished me with valuable help and suggestions, without which it would have been hardly possible for me to attain to any degree of success, as the subject is related in different ways to so many sciences other than psychology itself. My thanks are specially due to Dr. I. Miyake for aid in the measurement of velocity, and to Profs. G. Yamakawa, K. Osawa and K. Ikeda for furnishing me with many necessary points on electricity, the physiology of the nerves, and thermo-electricity.

which led me to the investigation of this problem; and though I do not claim to have succeeded in solving it, yet I hope the results given in the following paragraphs may give some clue to its satisfactory solution in the future.

About seven or eight years ago, it occurred to me that the nervous conduction might possibly be the transmission of a wave produced in a liquid contained in the nerve fiber. I then took a rubber tube nearly 20 feet long, and filled it with water to see the manner of transmission of the water wave. I struck one end of it, and the wave was transmitted very distinctly to the other end. I measured its velocity, and found that it was nearly 100 feet per second. Although the method of measuring was very imperfect, the velocity of the wave coincided so nearly with that of nervous transmission that I was induced to pursue the investigation further. But being unable to find any other point of analogy, I left the problem as hopeless, and did not take it up again for the next six or seven years.

In the spring of last year, this problem came again to my mind. I wondered whether we could not produce an action current in a rubber tube filled with water, and whether we could not produce inhibition in the tube. I made some apparatus specially for this purpose, and experimented on these points. The results obtained, though they differed somewhat from those I expected, were on the whole very satisfactory. I shall first describe the experiments made, then give their interpretation, and lastly compare the results with nervous conduction.

FIRST EXPERIMENT.

The aim of this experiment was to measure the velocity of transmission of a wave in a rubber tube.

The apparatus used for this purpose is shown in Fig. 1. In the first place, it is so arranged as to move the recorder *K* of Fig. 2. which forms a part of *H* in Fig. 1. The recorder is moved first when one end of the rubber tube *A* in Fig. 1. is touched, and secondly when a wave reaches the other end. One end of the rubber tube is fitted to a kind of tambour *B*, the other end to the U-shaped glass tube *I*, which is partly filled with mercury. *F* is a rod of ebonite at one end of which is a metallic button which is connected with the wire *G* by

FIG. 1.

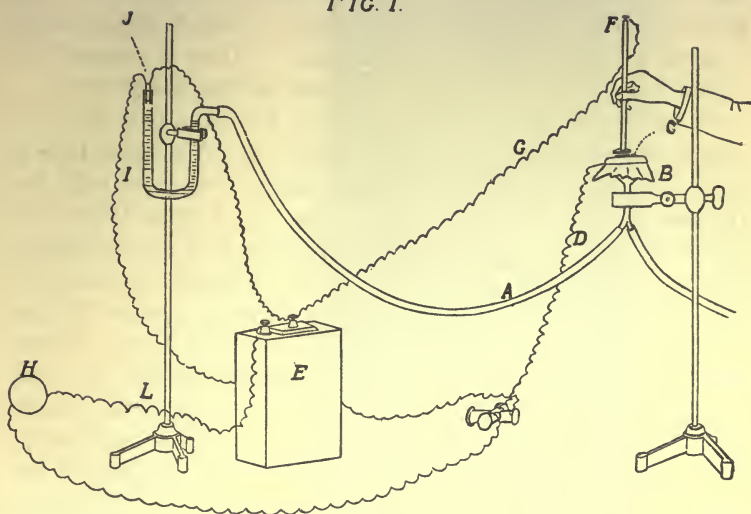
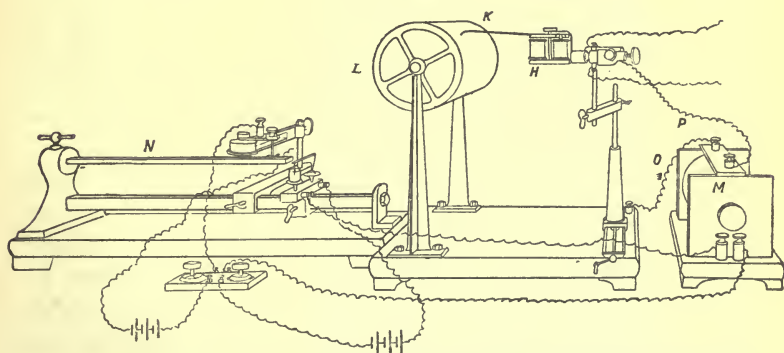


FIG. 2.



means of another wire passing through the interior of the rod and terminating at the other end. Thus a series consisting of a button and pieces of wire is connected with the battery *E*. *C* is a small piece of metallic plate which is connected with the other pole of the battery *E* through the wire *D*, the electro-magnet *H*, and the wire *L*. When the button touches *C* the circuit is closed, but is immediately opened again by lifting the button. By this closure, the recorder *K* in Fig. 2 is moved and a small curve is left recorded as shown in Fig. 3. When

the wave reaches the other end of the tube, the mercury in the glass tube rises and touches *J*, a piece of insulating material holding together the ends of two wires. Thus the second circuit is made, which moves the recorder for the second time.

Secondly, there is an arrangement for recording time by means of a series of spots on a straight line. The apparatus is shown in Fig. 2. *H* is an electro-magnet connected with the recorder *K*, corresponding to *H* in Fig. 1. *L* is a drum covered with smoked paper. *M* is an induction coil, the secondary current of which is connected with the recorder by means of wire *P* on the one hand, and with the drum by means of wire *O* and the stand supporting the drum on the other. Thus, when an interrupted primary current passes through the coil, a spark is produced between the recorder and the drum, which leaves a white spot on the smoked paper. This is Prof. Scripture's method of recording time. *N* is the interrupter of the primary current, devised by Prof. M. Matsumoto. This apparatus may be adjusted to interrupt any number of times in a second. In this experiment it interrupted fifty times a second. The interval between the spots on the record shown in Fig. 3, represents one fiftieth of a second. In this record of Fig. 3, we have 39.7 of such intervals between the first and the second contact. By changing the unit from one fiftieth to one hundredth of a second, Σ , the above number becomes 79.4 Σ .

Thirdly, we have to eliminate an error which comes from the inertia of the mercury. For this purpose I used a tube of a certain length, and then used another of just half that length. Representing by *X* the latent time produced by the inertia, by *a* the time for the transmission of the wave through the rubber tube, and by *C* the time including both of them, from the first experiment we have the equation

$$a + X = C \quad (1).$$

From the second experiment we have the equation

$$\frac{1}{2}a + X = C' \quad (2).$$

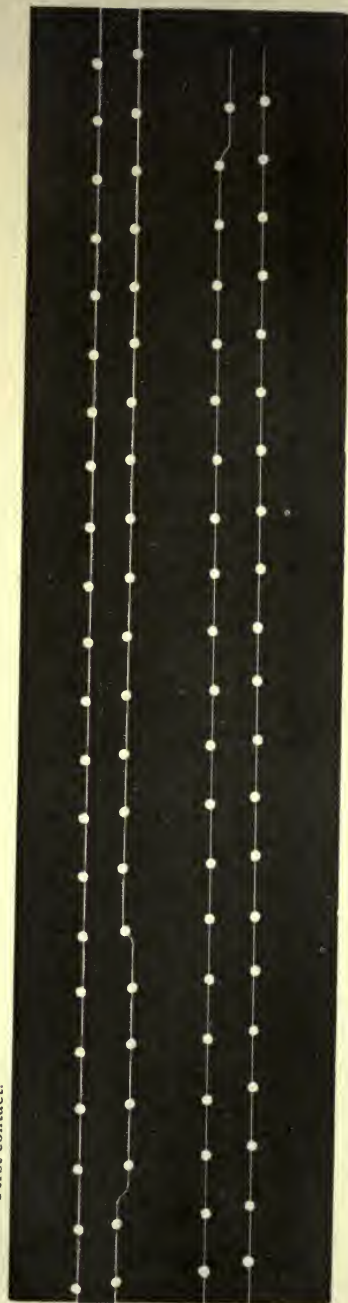
From the two equations we can find the value of *X*:—

$$X = 2C' - C.$$

Thus I obtained the value 3.36 for the latent time in this experiment.

Fig. 3.

First contact.



Second contact.

Fourthly, we shall show the results thus obtained, in the following tables:—

TABLE I.

Showing the velocity of wave transmission in different kinds of rubber tubes.

Kind of the tube.	Inner diameter in mm.	Length in m.	Time in seconds.	Mean variation.	Meters. per second.
Black tube.	6.	9.66	.796	.013	13.40
Do. (a little harder).	6.	6.53	.54	.9175	13.92
Do.	4.	6.66	.4488	.0105	16.21
White rubber tube.	6.0	1.85	.108	.0012	26.87
Do.	2.8	6.66	.0938	.0026	67.00

TABLE II.

Showing the influence of change of pressure, using the black tube.

Pressure Merc. Col. in cm.	Diameter in mm.	Length in m.	Time in seconds.	Mean variation.	Meters per second.
No pres.	6.	9.66	.8324	.01612	12.66
10.6	6.	9.66	.7692	.00896	13.76
16.75	6.	9.66	.7992	.00864	13.33
-9.00	6.	9.66	.8012	.00944	13.16

TABLE III.

Showing the influence of change of temperature, using the black tube.

Temperature in C.	Diameter.	Length.	Time.	M. V.	Velocity
Tem. of room	6.	9.66	.743	.0046	14.30
90	6.	9.66	.811	.005	13.00

TABLE IV.

Showing the influence of strength of stimulus, using the black tube.

Strength.	Diameter.	Length.	Time.	M. V.	Velocity.
Weak	6.	9.66	.796	.013	13.09
Strong	6.	9.66	.796	.0091	13.03

From these tables we learn the following points:

(1). That the velocity of the wave varies according to the nature of the rubber; and that it increases with the elasticity of the rubber, for we know that a white tube has greater elasticity than a black one, and a tube of smaller diameter than one of larger diameter.

(2). That the velocity increases with pressure up to a certain point, beyond which it decreases as pressure increases.

- (3). That the velocity decreases as temperature increases.
 (4). That the velocity is independent of the strength of stimulus, for the difference in our results is so small that we may consider it as probable error.

Interpretation.

The law of propagation of a wave in a liquid is as yet very little studied. It is conditioned by so many circumstances that it is very difficult to take them all into consideration. Prof. Maxwell gives us the following formula:

$$U^2 = EV$$

Where U is the velocity, E is the elasticity, and V is the volume of the unit mass (J. Clark Maxwell, Theory of Heat, p. 207). Moens gives us a more exact form of it in the following formula:

$$V_p = 0.9 \sqrt{\frac{gEa}{\Delta d}}$$

where E is the elasticity coefficient of the tube in grams per cub. cm., a is the thickness of the tube, d the diameter of the tube in cm., and Δ the weight of one cub. cm. of the liquid in grams (L. Hermann, Handbuch der Physiologie, IV B., 1ter Theil, S. 221). By comparing these formulæ, we see that, though they differ in their forms and one is more exact than the other, they agree in their essence. In Maxwell's formula, the square of the velocity is proportional to the elasticity and the volume of unit mass; while in Moens' it is proportional to the square root of the elasticity and inversely proportional to the square root of weight of unit volume; this is the same in its meaning as that of Maxwell, and has more conditions besides. Moens found by this formula and experiment that the velocity of wave propagation comes between 12 and 16 meters per second, with different intensities of pressure, and with different a and d . This result agrees with ours, except those obtained with the white tube, which is much harder than the black one. But Moens' formula agrees in approximation with our result in this, that the white tube, which has more elasticity and thickness, transmits the wave more quickly than does the black one, and that in comparing the white tubes, the one that has more elasticity and smaller diameter transmits it more quickly.

However, for some unknown reason, the smaller one transmits it a little more rapidly than the formula requires. There is another point which requires notice. In the experiment of Moens and Weber, a strong impression is transmitted more rapidly than is a weak one, while in our experiment a difference of intensity does not make any difference in the velocity of transmission. Probably in our experiment the difference of intensity was not enough to produce a difference in the velocity. Owing to the nature of our apparatus it could not be made greater.

Comparison of our Results with Nervous Conduction.

The velocity of nervous conduction was first measured by Helmholtz. It varies in different kinds of nerves, and the same nerve under different circumstances, *e. g.*, temperature. It ranges somewhere between 27 and 34 m. per sec. (Biedermann, *Electro-Physiology*, Engl. trans, Vol. 2, p. 60). Sometimes it is as quick as 90 m. (Hermann, *Handbuch der Physiologie*, B II., S. 23), and in some animals it ranges between 400 mm. and 1 m. (Biedermann, *op. cit.*, p. 61.) Thus we see that the range of the variation of velocity may come within that of the variation of velocity in the rubber tubes. In regard to the influence of pressure, I have nothing to say. As to temperature, my experiment showed a tendency opposite to that in the nerve. In my experiment the velocity decreased as temperature increased, while in the nerve it increases. Probably in the nerve the elasticity of protoplasm increases, while in the rubber tube the elasticity decreases, as temperature increases. Here the analogy fails, for one is living matter while the other is inanimate matter. Lastly, there is a disagreement of opinion among authorities as to whether the velocity of nervous conduction increases with the intensity of the stimulus. After all, the experiments of scientists seem to favor an answer in the affirmative. In my experiment, I could not find an influence of intensity, but in Moens' experiment there is such an influence. Thus the analogy between the nervous conduction and wave transmission may be retained here.

These facts, however, do not exclude the possibility of the

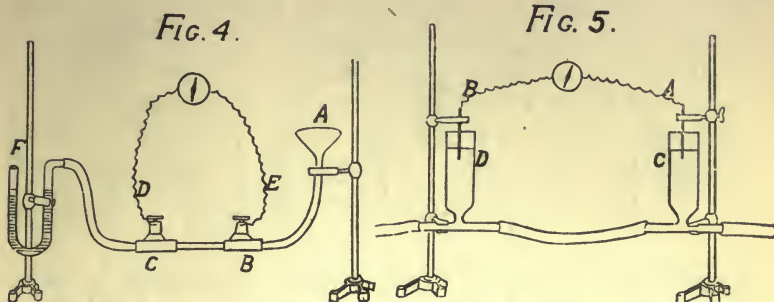
chemical explanation. For a certain chemical change such as crystallization of phosphorus is said to proceed with the velocity of 1 m. per second, while the explosion of dynamite goes several times faster than nervous conduction. Some chemical changes may proceed with just the same velocity as nervous conduction. It may be a kind of combustion, or a change in molecular arrangement. Or it may be a wave of protoplasmic contraction. It may perhaps be a combination of protoplasmic contraction and water wave. Finally, the analogy of velocity does not prove any thing definitely, for so many things have nearly the same velocity. This proves, however, that the nervous conduction may be explained on the hydraulic principle, if other analogies point toward the same conclusion.

SECOND EXPERIMENT.

The aim of this experiment was to see whether we could produce the so-called action current in a rubber tube filled with slightly acidulated water.

(a). For this purpose I made such an arrangement as is shown in Fig. 4. The general plan is the same as it is in Fig. 1. *B* and *C* are tubes of ebonite to which three rubber tubes are fitted to form one connected tube as in the figure, and they are connected with a galvanometer by wires. Thus we have a circuit completed, including the galvanometer and the liquid in a part of the tube. I noticed that a current existed, probably owing to the action of the acidulated water on the metal screwed on the ebonite. I noticed a striking change in the electric state when a stroke was given at one end of the rubber tube. This experiment did not give me anything new, but a suggestion for the next experiment.

(b). I tried to avoid the movement of the water at the point where it touched the metal. I made the arrangement shown in Fig. 5. *A* and *B* represent pieces of zinc plate which were held in position by clamps, and which touched, at their lower ends, acidulated water in the tubes *C* and *D*. At the bottom of the tubes, pieces of cotton were put in to prevent the wave from coming upward. By this arrangement, however, I not only failed to accomplish the end, but added a new disturbing cause, an electric current caused by capillary action in the cotton.



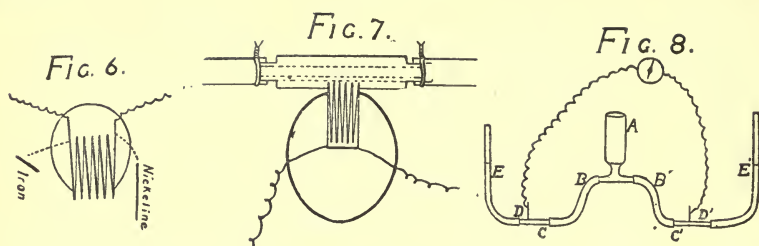
(c). Prof. Yamakawa, of the Electrical Engineering Department, suggested that a chemical change might occur between rubber and water. Therefore I substituted a glass tube for the rubber one, lying between *C* and *D* in Fig. 5; took the cotton from the bottom of the tubes; and, finally, substituted platinum for zinc. By this arrangement, I found that there was no electrical disturbance when a wave passed through the horizontal part of the tube. The question immediately suggested was, "What was the cause of the disturbance in the previous experiment?"

(d). In the arrangement of the experiment (c), I again inserted the cotton in *C* and *D*, and noticed that a current was produced when a wave passed through. Thus I was certain that the cotton was a cause of disturbance.

(e). Next I took off the cotton, and coated the inside of the glass tube lying between *C* and *D* with a mixture of ash and shellac, to give friction to the wave. I noticed an electric current when a wave passed through. Thus I came to the conclusion that this current was caused either by friction itself or by the heat thus produced, being a thermo-electric current in the latter case.

(f). I applied heat at the point *C*. I noticed an electric current produced. By applying heat at the point *D*, I noticed a current in an opposite direction. Thus I confirmed my impression that a thermo-electric current existed between the platinum and water. But it is another question whether heat enough for an electric current is produced in a rubber tube, when a wave passes through it.

(g). To answer this question I made a thermopile, on the



suggestion of Prof. Ikeda, of the Chemical Department, by joining together at their ends narrow pieces of iron and nickeline, as is shown in Fig. 6, where the heavy black lines represent iron, and light ones nickeline. The elliptic curve represents sealing wax fixed there to prevent one end from being exposed to changing temperatures. I inserted the other end in a hole made in an ebonite tube lying between the rubber tubes, through which the wave passed, as is shown in Fig. 7. The direct contact of the thermopile with the water was prevented by means of a thin rubber sheet. The terminals of the thermopile were connected with a delicate galvanometer. With this arrangement, I obtained the following readings, 500 being the zero point of the scale.

Before a wave passed through.	After a wave passed.	After a few seconds.
486.0	490.2	479.0
479.0	483.0	480.3
482.5	487.1	482.0
482.0	486.0	482.0
482.0	486.0	480.0
480.0	483.3	480.0

From this table, we see that there were changes of 3.3–6.0 mm., when a wave passed. From another experiment, I found that a deflection of the mirror of 1 mm. amounted nearly to $\frac{1}{250,000}$ of a degree Centigrade.

(h). I tried another experiment to see whether there was not a relation between the direction of the wave and that of the electric current. Fig. 8 shows the general arrangement for this experiment. *A* represents a glass tube whose upper end is opened. *B* and *B'* are rubber tubes. *C* and *C'* are glass tubes whose insides are coated with ash and shellac. *D* and *D'* are wires of platinum which go through the glass and touch the liquid in the tubes. *E* and *E'* are rubber tubes. When a

wave comes from either end, it stops at *A*. Therefore, a wave coming from the end *E* stimulates *D* only, and a wave coming from the end *E'* stimulates *D'* only. It made no difference, as to the direction of electric current, whether I struck the rubber tube *E* to send a wave from *E* to *B*, or the tube *B* to send a wave from *B* to *E*. I concluded, therefore, that the direction of the electric current depended upon which electrode was excited, and not upon the direction of the wave.

(i). These results suggested the question whether we could not produce a thermo-electric current in a nerve. To answer this question, I made the following experiment. I took a sciatic nerve of a frog, and applied unpolarizable electrodes at two points somewhat distant from each other. Pouring on hot water at one of the points, I noticed such a deflection of the galvanometer as to show an electric current passing through the galvanometer from the hot to the cold point. Next I used two pins as electrodes, and warmed one of them before applying them to the nerve. I found the same result as before.

I wished to know what would happen, if the nerve itself were warmed instead of an electrode, but I could not succeed in this experiment, owing to the mechanical difficulty of warming a nerve. Therefore I warmed one part of my own body and applied an electrode to it, while the other electrode was applied to the colder part of the body. I found an electric current passing through the galvanometer from the colder part to the warmer part.

(j). Does a nerve produce heat when a stimulus passes through it? To answer this question, I took a sciatic nerve of a middle-sized frog, together with a muscle, and applied the thermopile at a certain point in the nerve, while two electrodes gave a shock at the end of the nerve further from the muscle. In this experiment, one shock gave no deflection in the galvanometer, but when I gave a faradic current continuing half a minute, the mirror was deflected 2 mm.

To make sure that the heat observed at the point adjacent to the thermopile was not the heat transmitted from the point at which the stimulating electrodes were applied, I had put a piece of iron between these points to intercept it. Thus the deflection of the mirror must be entirely attributed to the heat produced at the point next the thermopile.

The galvanometer used in this experiment was a Deprez-d'Arsonval mirror galvanometer, read with a telescope and scale in the usual way. The distance of the telescope from the mirror was nearly 1.5 m enabling me to read a very small deflection of the mirror. The internal resistance of the galvanometer is nearly 400 Ohms, and 8.5×10^{-10} ampere produces a movement of the scale of 1 mm.

Interpretation.

When any two electric conductors are brought in contact, a difference of potential is produced between the two. If we dip the ends not so touched in acidulated water, a current is produced through the water from one to the other. If the conductors be copper and zinc, the current passes from the zinc to the copper through the water and from the copper to the zinc at their points of contact outside. Instead of directly connecting copper with zinc, if we connect them by means of a wire, supposing the temperature to be the same everywhere in the circuit, a current goes from copper to zinc through the wire. If the temperature is raised as, for instance, at the point of contact—a current called a thermo-electric current is produced, whose direction differs according to the nature of the conductors thus connected. Prof. Wiedemann divided all kinds of conductors into two large classes, the first class composed of metallic conductors, and the second of electrolytic conductors. The former produces electricity without any chemical change, and includes metals, a peroxide, and a compound of metal and sulphur. The latter needs chemical decomposition to produce an electric current, and includes in it all kinds of salts in the widest sense of the word, water, and others. (Gustav Wiedemann, *Die Lehre von der Electricität*, 1ter B., S. 191.)

Mere contact of two conductors produces a difference of potential, but the energy of the current may come from chemical decomposition. Electric currents and chemical changes are very intimately connected, but the connection is not essential. A current may be produced without any chemical change, as is the case with a thermo-electric current. Prof. Balfour Stewart says, "It was discovered by Seebeck that if a circuit composed of two different metals soldered together have one of

its junctions heated, an electric current will be produced." He says again, "If a compound circuit be made with any two metals in the following list, the positive current will go across the heated junction from the metal nearest the top to that nearest the bottom of the list:—

- | | | | |
|-------------|--------------|------------|------------------|
| (1) Bismuth | (4) Tin | (7) Silver | (10) Antimony |
| (2) Nickel | (5) Copper | (8) Zinc | (11) Tellurium." |
| (3) Lead | (6) Platinum | (9) Iron | |

(Balfour Stewart, *An Elementary Treatise on Heat*, 2nd Ed. pp. 157-58.)

A thermo-electric current is produced not only between metals but between a metal and an electrolyte, and also between electrolytes themselves. Prof. Wiedemann gives the following facts: When two platinum plates connected with a galvanometer, one of which is heated, are dipped in cold water, the heated one becomes positive toward the cold one (compare experiment (i) above). We may have the same result by dipping the two plates first and then pouring hot water on one of them. A hot platinum wire becomes positive toward a cold one in the following liquids:—sulphuric acid, nitric acid, ammoniac, solution of magnesium sulphate, tin chloride, copper chloride, iron chloride, and some other liquids. It becomes negative in the following liquids:—chlorhydric acid, oxalic acid, vinegar, potash, potassium carbonate, and some other liquids—(Gustav Wiedemann, *op. cit.*, 2ter B., S. 30. 304-5). Prof. Wiedemann says, in another place, concerning the thermo-electric current produced between two different electrolytes, that there is a doubt as to whether heat is the direct cause of the current or whether heat produces a chemical change which is the direct cause. And he gives various facts to show that there are many cases in which heating of the point of contact of the two electrolytes produces a current (Wiedemann, *op. cit.*, 2ter B., S. 316-20). And again he says, there is a phenomenon called the electric current of a stream of fluid. When a current of liquid passes through a partition of porous matter, an electric current is produced. And when a liquid flows through a tube, as small as 0.949-0.152 mm. in diameter and 10-55 mm. long, an electric current is produced whose direction coincides with that of the liquid, and whose intensity is nearly proportional to

the difference of pressure, by which the stream of liquid is caused. (Wiedemann, *op. cit.*, Iter B., S. 982-993.)

By comparing these facts with the experiment (b), we see why the cotton was a cause of electric disturbance. By referring to experiments (e), (f), and (g), we see that there is a certain relation between heat and the electric current, but the fact just described, suggests a doubt as to whether heat was the only cause of the current in these experiments, or whether the current was partly caused by heat and partly caused by the motion of the liquid in the tube. By experiment (h), we see, however, that the direction of the current does not depend on that of the liquid motion, as it must do if the current depends on liquid motion. Hence I am convinced that the electric current in these experiments was a thermo-electric current.

Comparison of our Results with the Electric Current in the Nerve.

We are not certain as to whether there is any current in the nerve when it is in a state of rest, though we know that there is a current in the nerve when an impulse passes through it. This current is called an action current or a negative variation, for the point where the impulse is passing becomes negative toward all the other points. Concerning the nature of this current we have not any definite knowledge, in spite of the attention paid to it by many scientists. The problem, in which the scientists' attention is focused, is whether the action current is essential to nervous activity or not. I do not wish to enter upon a physiological discussion, but I believe that the action current is explicable as a thermo-electric current produced between the two points of the nerve where the two electrodes touch it (compare the experiments (i) and (j)).

THIRD EXPERIMENT.

The purpose of this experiment was to find whether we could produce in a rubber tube a phenomenon similar to inhibition in the nerve. For this purpose I made the arrangement shown in Fig. 9. Fig. 10 is a plan of its most essential part. *A* is a reservoir having a piston to regulate the pressure of water in it. There are six openings, four on the sides of the reservoir itself and two in the piston, to each of which a stopcock is fixed, which may be closed or opened at pleasure. One of

FIG. 9.

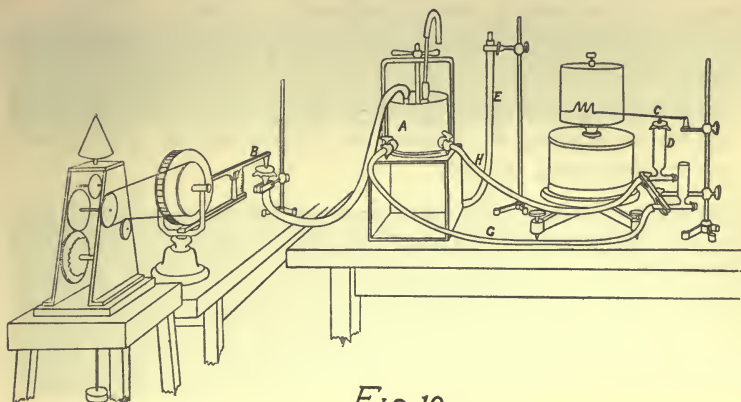
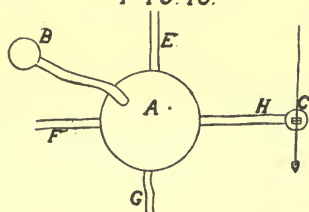


FIG. 10.



these openings is for the pouring in of water. Another is to transmit a wave produced at *B* to the reservoir by means of a rubber tube. *E*, *F*, *G* and *H* are rubber tubes which lead the wave away from the reservoir. When *E*, *F*, and *G* are closed, the tube *H* will lead off the wave. *D* is a glass tube whose upper end is covered with a thin rubber sheet which moves freely as the water in the tube moves. A small piece of cork on the rubber sheet supports a recorder. When a wave arrives at *D*, the recorder is moved and leaves a record on the smoked paper. This record is a series of curves with a certain height. When *E* was opened together with *H*, the height of the curve was diminished as a part of the wave was lead off by *E*. When *F*, which was a little larger than *E*, was opened, the height of the curve was still more diminished. By measuring the height of each curve, I obtained the following results.

Standard curve.	E. F. G. curves resp'y.	Differ. or quantity led off.	Diameter of tubes.	Ratio bet. the quant. and the diameter.
12.	8.	4.	9.	2.25
12.	7.	5.	11.5	2.3
12.	5.	7.	16.5	2.35

From this table we see that the quantity of wave led off is nearly proportional to the diameter of the tube.

Next I studied the relation of the wave led off to the length of the tube. By using a tube 1.5 m. long, then one of half its length, then one of one-fourth its length, and finally one of one-eighth its length, I obtained by measurement and calculation the following results.

Standard curve.	Height of each curve.	Difference or quantity led off.	Length of each tube.
11.8	8.5	3.3	1500.
11.8	6.5	5.15	750.
11.8	6.05	5.75	375.
11.8	5.3	6.5	187.5

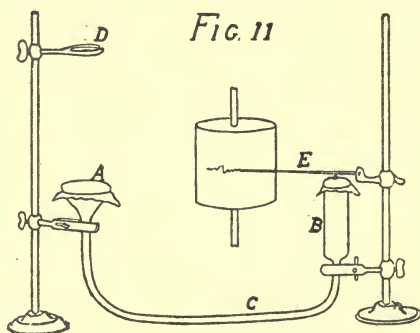
From this table we see that as the length of the tube decreases the quantity led off increases, but we cannot find any definite law. I may formulate these results in the following propositions:—

(a). The quantity of the wave led off by any tube is nearly proportional to its diameter.

(b). The quantity of the wave led off by any tube increases as its length decreases.

FOURTH EXPERIMENT.

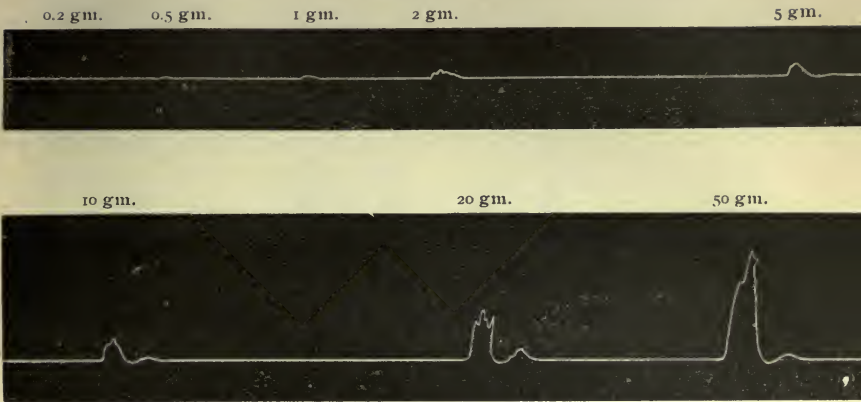
I tried to see how sensitive was the transmission of a wave by a rubber tube. For this purpose I made the arrangement shown in Fig. 11. *A* is a funnel whose upper end is covered



with a rubber sheet, while its lower end is connected with the rubber tube *C*, which is connected at the other end with the glass tube *B*. Different weights dropped from *D* on *A*, pro-

FIG. 11A.

Using a Rubber Tube whose diameter was 6 mm.



Using a Rubber Tube whose diameter was 9 mm.

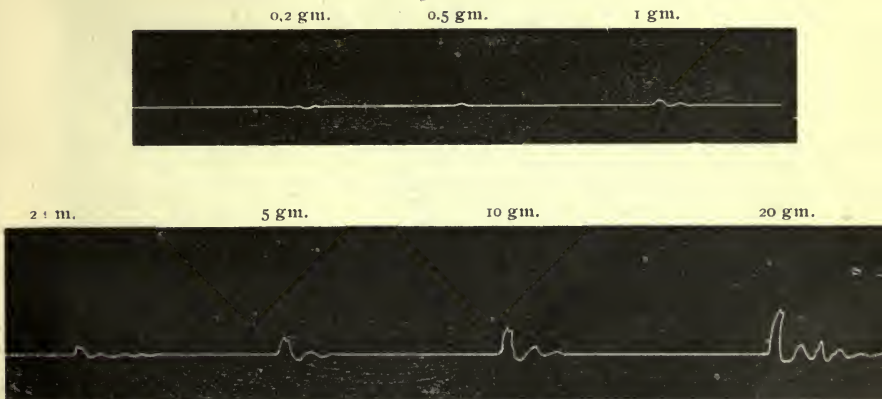
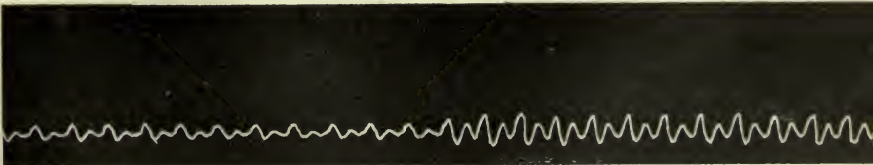


FIG. 12A.



(over.)

Fig. 15

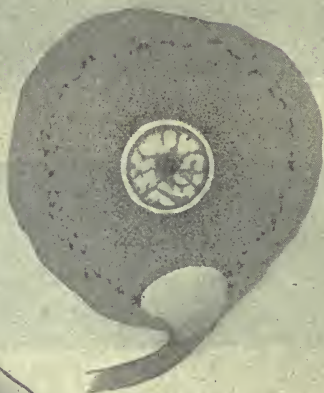


Fig. 17

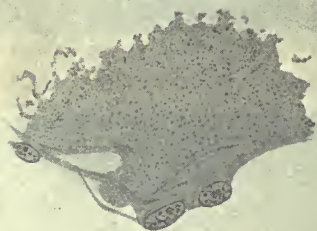


Fig. 19

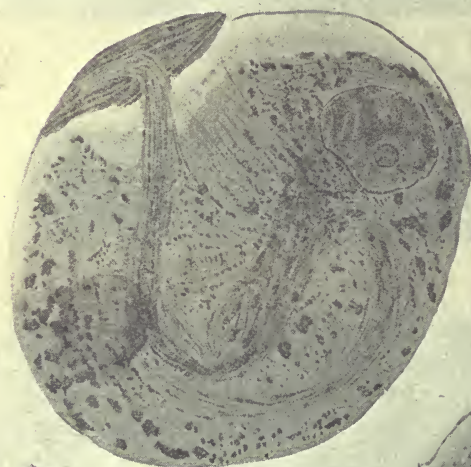


Fig. 20



Fig. 18



Fig. 16

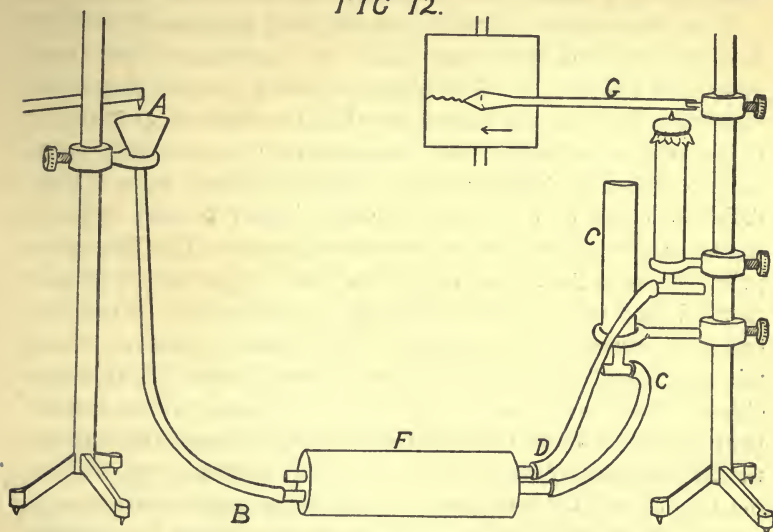


duced different effects on the record *E*. Fig. 11a represents the results thus obtained.

FIFTH EXPERIMENT.

I tried to see the relation between the law of isolated conduction of stimulus and the paradoxical contraction; (Compare, I. Rosenthal, *Allgemeine Physiologie der Muskeln und Nerven*, 2te Aufl., S. 111 und 308). For this purpose I made the arrangement shown in Fig. 12. *B* is a rubber tube which

FIG 12.



is kept in contact with another tube *D*, by means of a larger tube *F*. *B* is excited at the end *A*, while the other end *C* is opened. A small amount of vibration was transmitted to the tube *D*, which was recorded by means of the recorder *G*, as is shown in the first half of Fig. 12a. When I pressed the tube *B* at a point between *F* and *C* to close the tube at *C*, the wave in the recorder became larger as is shown in the last half of the figure. It made no difference whether I gave the stimulus at *C* and closed the end *A*, while the other things remained the same. I may compare the case where *C* is opened to that of isolated conduction, while the other case where it is closed to the paradoxical contraction. For in the latter case a branch

of the nerve is cut off and thus the opening of the cut end must have been closed by the contraction of the protoplasm.

CONCLUSIONS.

The nervous system consists of nerve cells and fibers. The developmental unit of the system which comprises ganglion cell, neuraxon, dendrites, and their ramifications, is called a *neuron*. The neuraxon is an efferent cell-process, while dendrites are afferent processes. The aggregation of these units is held together by a network of fine fibrils called neuroglia.

If we take out one of the nerve cells and examine its interior structure, we find two constituents in it, structural and non-structural (Fig. 15). The former is called the cell-corpuscles of Nissl. The smaller corpuscles take the form of granules or fibers, while the larger ones, comparatively, are spindle, cone, or hood-shaped. These shapes differ in different cells. Thus different types of cells are formed. Nissl divides cells in the central nervous system into two classes. The first comprises those cells whose cell bodies are large and distinctly marked, and whose nuclei are entirely surrounded. These are called "somatochrome cells." The second comprises those whose cell bodies are small and are mostly occupied by a nucleus. Most of the nerve cells belong to the first class, which is again divided into the four following subclasses: the types of net-formed arrangement, that of striated arrangement, that of net-formed striated arrangement, and that of granulated arrangement. Recently, however, he has given the following division as a better one.

- (1). A group of striated arrangement.
- (2). A group of granulated arrangement.
- (3). A group of those cells which resemble one another and are not comprised in any of those yet enumerated.
- (4). A group of net-formed arrangement, which is to be divided into many subclasses.

There is a question as to whether the cell-corpuscles of Nissl are a natural structure, or a product of chemical treatment or other changes occurring in it after death. Held thinks that it is produced after death, for it does not appear in a cell immediately after death. Lenhossék thinks, however, that it is not

necessarily produced after death, for it appears in a fresh cell, at least, of the spinal ganglion. According to Marinesco, Nissl's corpuscle is an original formation, and is a source of energy. For this reason he calls it *Kinetoplasma*. Thus we see that there is as yet no definite idea concerning Nissl's corpuscle.

Moreover, we have a very imperfect idea concerning the nonstructural substance in the cell. This substance receives various names from different points of view. It is sometimes called intermediate substance, achromatic substance, ground substance, or ground mass (Held) of nerve-cell protoplasm, and sometimes it is called spongioplasm from its fine mesh constitution. *Nonstructural* is not a proper name, for recently its structure has been made visible. Lenhossék, however, discovered that the mesh-like appearance is due not to a net work of fine fibrils, but to an aggregation of fine granules. Bütschli and Held, on the other hand, affirm that this appearance is due partly to an aggregation of fine vacuoles which is very likely a result of chemical treatment, and partly to chains of fine granules which lie in the vacuoles (Fig. 16 and 17). They think that these structures are not limited in the cell body but continue to dendrons and axis cylinders. These items of information concerning the anatomy of nerve cells are taken from a work of Goldscheider and Flatau (A. Goldscheider und E. Flatau, Normale und pathologische Anatomie der Nervenzellen.)

According to Bühler, the fibriform structure of the axis cylinder takes a winding course, as is shown in Fig. 18. Sometimes the other end comes back to its point of original entry as is shown in Fig. 19. Or sometimes it enters at one place and goes out at another as is shown in Fig. 20. (Dr. med. Anton Bühler, Untersuchungen über den Bau der Nervenzellen.)

I tried an experiment to see whether I could produce such a winding appearance mechanically by means of an hydraulic wave. I took a properly shaped glass tube and connected it with a rubber tube which ended in a metallic funnel covered with a rubber sheet for receiving a series of strokes. I filled the whole tube with glycerine in which were short pieces of silk thread. Then I produced a series of waves in succession.

The threads in the glycerine took a winding shape in the glass tube.

Lastly, in regard to the question whether a nerve cell undergoes any mechanical change when an impulse passes through it, we have not any decided knowledge. The investigations of many scientists seem to show us that the activity of the nerve cell is accompanied by an increase of volume of the cell body, and a decrease of the chromatic constituent (Goldscheider und Flatau, *op. cit.*, S. 35).

The facts above described do not as yet answer the question of nervous conduction. Scientists of the present day think that nervous conduction is due to a chemical action caused by an organic function. But we do not know the details of the manner of the chemical action. Thus there is a possibility of trying some explanation other than chemical. For this reason I propose an hydraulic explanation. It supposes that nervous conduction is a transmission of a water wave in a protoplasmic tube and that the protoplasmic tube not only helps the transmission by its own elasticity but is excitable at any point by means of a stimulus directly applied to it. The wave is, of course, equally transmitted in both directions. Moreover this theory does not necessarily require a continuity of the path of conduction. Mere contact of tubes is enough to transmit a stimulus (compare the Fifth Experiment). Mere presence of the watery medium between two tubes is likewise enough for the purpose. The explanation has, however, its own difficulties. We cannot tell whether a nerve fiber is a sort of tube of protoplasm filled with a fluid or semi-fluid. If it is true that the fibrous appearance in the axis cylinder is a post-mortem product, we cannot infer anything from this as to the nature of protoplasm in life. I am not ready to explain everything on the hydraulic principle. If the water wave is to explain nervous conduction, it must be supplemented by the contraction of protoplasm, which forms the tube, to account for excitability. As to further information on the nature of conduction, we must wait for future discovery.

There are two large classes of psychical phenomena which are very conveniently explained under the supposition of a protoplasmic tube. They are the phenomena of attention and

inhibition. According to our hypothesis, these two phenomena are looked upon as two aspects of one and the same phenomenon. Attention is one aspect of an activity where impulses centralize, while inhibition is the other where impulses are turned away. If there is attention in one place, there must be inhibition in another. If any part of the brain becomes active, the nerve cells and fibers of that part, increase in volume, and impulses are gathered there in consequence; this coincides with a result of our investigation. In other words, impulses come together, and focus at a point where activities already exist. Thus they keep their attentive state until the cells and fibers of that part become tired, when the attention passes to another point.

In regard to the nature of inhibition I have not as yet a decided opinion. It would be proper, however, to distinguish two kinds of inhibition. (1). There is, so to speak, an inertia of the nerve, that is, it needs to heap up some energy before it is called into activity. The energy, thus spent, can be said to have been inhibited. For this reason, when a nerve cell is in a state of activity, it transmits a comparatively small amount of the energy newly arrived. (2). By hypothetically accepting the contraction of neuroplasm when stimulated, and, as its consequence, a little widening of the nerve fiber, we may affirm that those cells together with dendrites and fibers, which are already in a state of activity, have less resistance to the conduction of a stimulus than those at rest. If this affirmation is correct, a stimulus coming from the periphery to a center would be led off toward such a part as is already in the state of activity. Thus when a star-fish put upside down tries to recover its proper position by means of one of its legs helped by a few neighboring ones while the other legs are at rest, it accomplishes its purpose. Some legs seem to be at rest as a consequence of the concentration of the excitation in the other legs. Therefore, if the central connection of the nerves were cut, all the legs would act simultaneously. Again when we direct our attention earnestly to a special point, and therefore only one part of our brain is in a state of activity, the other parts are almost insensible to a stimulus. Thus we try to depress the sensation of pain in surgical operations by turning

attention somewhere else. The inhibitory influence of the vagus on the heart may be considered as a consequence of its expansion caused by a stimulus, and of its leading away the stimuli originated in the heart itself. In these cases, the stimulus is led off toward the point which is active. This is not really inhibition, but only turning off the stimulus.

In the reflex action of a frog whose brain has been cut off, when one leg is stimulated, that leg is moved. When the intensity of stimulus is increased, the other leg is moved, and by increasing it still further, the arm on the same side is moved, and so on. These facts seem to coincide with our experiment, on the relation of the length of tube to wave conduction.

Finally, I might say that a chemical action may explain these phenomena just as well as an hydraulic principle. But, in that case, the former must work according to the same law as the latter, for attention and inhibition are sufficiently explicable under the hydraulic supposition. As to the special application of this principle to psychology, I must defer what I have to say to a future work.

THE RELATION OF MOTOR POWER TO INTELLIGENCE.¹

By Professor T. L. BOLTON, University of Nebraska.

The application of the theory of evolution to the field of motor development has received wide recognition in the literature of several lines of investigation, and may be looked upon as one of the most vitalizing conceptions that has come into modern physiology, psychology and pedagogy. The theory of motor development is the product of it. The first suggestions of this came from Ross,² who, in his 'Diseases of the Nervous System,' distinguished between what he called the fundamental and the accessory among the muscles of the human body. Hughlings-Jackson followed up this suggestion by his doctrine of three levels in the development of the nervous system, which he applied in the interpretation of different forms of nervous and mental diseases.³ Recent applications of these suggestions have been made in the fields of psychology. For the opening up of this line of investigation we are chiefly indebted to President W. L. Bryan and Professor John A. Hancock.⁴ These investigators furnished experimental proof of what had been suggested on the side of nervous diseases. Further evidences have been found in other lines of work. The study of cell development, investigations relating to brain

¹ This work was done in the psychological laboratory of the University of Nebraska. Mr. Thomas F. Butcher, principal of the High School at Ashland, Neb., assisted in taking the observations. To his patience and skill in managing children much of the success of the work is due.

² Ross: *Diseases of the Nervous System*. London.

³ Hughlings-Jackson: *Med. Chir. Trans.*, 1872.

⁴ W. L. Bryan: *On the Development of Voluntary Motor Ability*. *Amer. Jour. Psych.*, Vol. V, p. 125.

F. B. Dresslar: *Some Influences that affect the Rapidity of Voluntary Movements*. *Am. Jour. Psych.*, Vol. IV, p. 514.

J. A. Hancock: *A Preliminary Study of Motor Ability*. *Ped. Sem.*, Vol. III, p. 3.

localization, observations upon the process of degeneration in old age, and in progressive paralysis, and more extended experimental studies all confirm the hypothesis that motor power is a matter of slow growth, and that full maturity is reached only after growth in stature is complete.

The theory of motor development may be stated in some such form as this. The muscles of the human body are not all equally important and fundamental; they form a graduated series, with the most fundamental at the one extreme, and the most accessory at the other. The order of development is from the fundamental to the accessory. The most fundamental develop and become functionally active first, and the others in the order of their importance afterwards, the most accessory coming to full maturity late in life. Motor power is not a simple phenomenon; it is capable of being analyzed into a number of elements, the most important of which are rapidity of voluntary control, steadiness and precision of movement, variety of actions, and quickness, strength, and endurance of contraction. Development does not take place in all these respects at the same rate, nor are they all of equal importance. Some of them may be more easily experimented upon than others, and the ease of experimentation depends upon the kind of reagents that are chosen. The strength of grip, for instance, is most easily experimented upon, but the results are subject to the widest variations through practice and accidental causes, so that it is very difficult to say when a probably valid result has been obtained. Measures of endurance are equally variable. Quickness of movement requires a complicated apparatus, and that makes statistical work impracticable. No one seems to have found a method of measuring the variety of actions of which any member, or group, is capable. So that, for studies of motor power only three factors remain which can be easily studied with confidence that the results for all subjects are fairly uniform and comparable; these are rapidity of voluntary control, and steadiness and precision of movement. Great difficulties are undoubtedly encountered in investigations of these, and the observations leave much to be desired in the way of uniformity and general trustworthiness; but still we feel a fair degree of confidence that the final outcome will be

a close approximation to the real status of the persons examined.

The application of this theory to feeble mindedness and arrested growth was but a short step, and observations were soon made showing that defective children differed from the normal chiefly in their power to move. Mental development and motor power go hand in hand. From this general statement the passage is easy to the supposition that tests of motor power may be used as measures of intelligence or mental alertness. Schoolmen have long discredited the old-fashioned methods of examinations, and the examinations are slowly giving way to the prevailing distrust. The search for a substitute for the examination—for there is a real need for something of the kind, more than the mere criterion of age, in classifying pupils in the schools,—has not been rewarded by great success. The demand calls for a method of classifying pupils that will take account as well of the natural aptitude or capacity of the pupil for learning as of the attainments already made. Tests of physical endowment and of general healthfulness of body seem to offer the most promise of finding what is wanted.

As a consequence of these general suggestions, application was made to the Superintendent of Schools in Lincoln, Nebraska, for opportunity to try certain well-known, and some new, tests upon the children of the public schools. Two classes of children were chosen: those from the best wards in the city, where the general home surroundings were the most wholesome and cultivated, and others from the lower wards, where the largest percentage of poor foreigners is to be found, and where the hygienic conditions of the home as regards food, clothing, sleeping, air, and light are bad. Lincoln is what might be called a country city. The best people are good representatives of the best class of Americans, and the poor people are a mixture of various nationalities of foreigners, mostly Russian Germans. There is no real tenement or slum class, whose poverty is squalid and distressing. The children are American born, and the worst that can be said of the parents is that they do not know how to live and are indifferent to a comfortable standard. When the College Settlement was started among the people, they took interest and showed improvement.

Tests of rapidity of voluntary control, of steadiness in standing, and of steadiness and precision in moving either hand, were chosen as those most easily made, because most quickly learned through imitation, and most likely to give trustworthy results. They seem, also, to give less fluctuating results in successive trials than do other tests. For the first test an old clock fitted with a key for tapping with the finger was used. It was very similar to the one used by Bryan in his test of the school children in Worcester, Mass. Each child was given five trials with either hand to find the number of taps that could be made in five seconds. The child was shown how to do the work, and then allowed to make a trial with either hand before the actual observations were taken. In taking the five trials the hands alternated.

For testing the precision and steadiness in moving either hand, a new apparatus was constructed. It consisted of a number of strips of brass one-half inch wide, and eight inches long. These were arranged parallel to one another at different distances apart, and were all connected with one pole of a battery. They were fastened only at the ends so that a needle set in the end of a glass rod might be passed between them. The needle was connected with the other pole of the battery and a relay signal placed in the circuit. When the needle was brought into contact with the brass, this fact was indicated by the signal. The several distances between the brass strips were one-half, three-eighths, one-quarter, and one-eighth of an inch. The test consisted in passing the needle successively through these openings, beginning with the widest, from one end to the other, either downward, upward, to the right or to the left, and with both hands, the hands being alternated in the several trials to avoid fatigue. The record was kept in the number of times the relay signal sounded, indicating contact between the needle and the brass. With some children the amount of unsteadiness was considerable, so that the figures representing the number of contacts are not entirely accurate, because the sounds could not always be readily counted. The disturbance caused by the relay signal may be avoided by holding the fingers upon the poles of the signal and thus receiving the shock through the hand. If the battery is not strong the

successive shocks can be easily counted, but the child needs to know by the sound as well as by sight that the needle rests against the brass.

For the third test steadiness in standing was chosen. The ataxiagraph devised by Crichton-Browne,¹ and used by many others, was employed. This instrument has been used more often as a test of nervous health and strength than of motor power. For the former purpose it is probably better adapted, as some of our observations will show, and yet nervous weakness is so much a matter of slow and imperfect growth that most diseases of the nervous systems in children may be regarded as simply manifestations of a disturbance of the processes of development. If this be a true statement of the case, then, where the instrument shows a lack of nervous control, the subject may be looked upon as a case of slow growth or retarded development. However, cases of manifest nervous and mental weakness show certain characteristics that are not clearly found in any of the stages of motor development. This matter will come up again when a discussion of the observations themselves is reached.

The purpose of the experiment was to determine whether tests of motor power showed the same backwardness and deficiency which had already become apparent in the school work. About sixty children from the lower wards were tested and compared with an equal number from the best families in the best wards. In the following table will be found the distribution, according to grade in school work, of the children examined.

Grade.	1st.	2nd.	3rd.	4th.	5th.	Grade not known.	Total.
Poor children,	24	15	7	—	—	13	59
Good “	—	6	27	15	4	—	52

This shows that the better children average just about two grades higher than the poorer. Among the poorer a class of ‘grade not known’ is given. This includes those children who did not know their grade. When inquiry was made of the teachers we were told that a considerable number of children had been in the school several years, making little or no progress, so that they were practically without grade. Such chil-

¹Crichton-Browne: *The Nervous System and Education*. London.

dren were simply allowed to stay in school, as the school was a better place for them than the street; besides, the law prescribes that they shall be sent to school. The teachers deal with them in the best way they can, but have little hope of their making any progress. These children, with four exceptions, reported their ages as either eight or nine years. Some of them were quite uncertain about it, and not a few found difficulty in understanding what was wanted when the ages were asked for. No attempt was made to pick out the poorest among those found in the lower ward schools; all the children in the school who could be persuaded to take the tests were examined. A number who were manifestly weak and feeble were rejected, and several others seemed so dull that they could not be made to understand what was wanted of them. Their attention could not be kept upon the work long enough to get satisfactory tests. Some allowance must be made for a few because they did not readily understand the English language; yet where they were reasonably bright, they understood quickly through imitation what was expected of them.

The following table gives the results of the tapping tests. It appears in three parts: the first shows the comparisons between the right and left hands of pupils of the same age and of the classes good and poor; the second shows the comparisons between the good and poor of the same ages and for the right and left hands; and the third shows the comparisons between pupils of eight and nine years of age for the classes good and poor and for the right and left hands. As stated above, five trials of the number of taps that could be made in five seconds of time were taken. The figures in the separate columns represent the average number of taps for the pupils without regard to grade or sex. 'R. H.' and 'L. H.' stand for right and left hands respectively.

TABLE I.
Tapping Experiment. First Part.

Trials.		1st.	2nd.	3rd.	4th.	5th.
Good. 9 yrs.	R. H.	29.76	30.21	30.31	30.93	31.39
"	L. H.	26.48	26.90	26.93	26.68	26.80
Difference		3.28	3.31	3.38	4.27	4.59 = 8.83

Trials.		1st.	2nd	3rd.	4th.	5th.
Good.	8 yrs. R. H.	27.60	28.80	28.50	28.80	29.00
"	" L. H.	24.00	24.20	24.90	24.00	24.80
Difference		3.60	4.60	3.50	4.80	4.20=20.80
Poor.	9 yrs. R. H.	26.42	28.50	28.08	27.75	27.00
"	" L. H.	24.95	23.67	23.73	23.36	23.12
Difference		1.47	4.83	4.35	4.39	3.88=18.92
Poor.	8 yrs. R. H.	26.25	27.75	26.83	27.91	27.40
"	" L. H.	23.33	23.17	24.16	23.10	23.44
Difference		2.92	4.58	2.67	4.81	3.96=18.94

Second Part.

Good.	9 yrs. R. H.	29.76	30.21	30.31	30.93	31.39
Poor.	" "	26.42	28.50	28.08	27.75	27.00
Difference		3.34	1.71	2.24	3.18	4.39=14.85
Good.	9 yrs. L. H.	26.48	26.90	26.93	26.68	26.80
Poor.	" "	24.95	23.67	23.73	23.36	23.12
Difference		1.53	3.23	3.20	3.32	3.68=14.96
Good.	8 yrs. R. H.	27.60	28.80	28.50	28.80	29.00
Poor.	" "	26.25	27.75	26.83	27.91	27.40
Difference		1.35	1.05	1.67	.89	1.60= 6.46
Good.	8 yrs. L. H.	24.00	24.20	24.90	24.00	24.80
Poor.	" "	23.33	23.17	24.16	23.10	23.44
Difference		.67	1.03	.74	.90	1.36= 4.90

Third Part.

Good.	9 yrs. R. H.	29.76	30.21	30.31	30.93	31.39
"	8 " "	27.60	28.80	28.50	28.80	29.00
Difference		2.16	1.41	1.81	2.13	2.39= 9.90
Good.	9 yrs. L. H.	26.48	26.90	26.93	26.68	26.80
"	8 " "	24.00	24.20	24.90	24.00	24.80
Difference		2.48	2.70	2.03	2.68	2.00=11.89
Poor.	9 yrs. R. H.	26.42	28.50	28.08	27.75	27.00
"	8 " "	26.25	27.75	26.83	27.91	27.40
Difference		.17	.75	1.25	-.16	-.40= 1.61

Trials.			1st.	2nd.	3rd.	4th.	5th.
Poor.	9 yrs.	L. H.	24.95	23.67	23.73	23.36	23.12
"	8 "	"	23.33	23.17	24.16	23.10	23.44
Difference			.62	.50	.43	.26	-.32 = .61

Let us look at the first part of the table. The figures at the right of the table, representing the sums of the differences for the five successive trials when the right and left hands are compared, show that there is about the same difference between the hands for both good and poor at both ages—eight and nine. The hands of the eight year old children tend to differ more than those of the nine year old. This may, however, be only accidental, as the differences are too small to place any reliance upon. In the second part, where the comparison is made between the good and the poor, some interesting results are shown. The good children of nine years of age both with the right and left hands differ more strikingly from the poor of the same age, than the good of eight years do from the poor of the same age. The figures that represent these differences are 14.85 for the right and 14.96 for the left hand for the nine year old children and 6.46 for right hand and 4.90 for left hand for the eight year old children. The difference which is small at eight is increased at nine, and had the tests been carried farther we might reasonably expect a still wider difference. A few children of ten years could have been got, but most children who make no progress in school work by that age become irregular in attendance and finally drop out. It may be that some significance is to be attached to the fact that at eight years of age the right hands of the good and poor differ more than the left hands do. This is shown by the difference between 6.46 and 4.90. The fact that the good and poor differ more at nine years of age than at eight years shows clearly the phenomenon of arrested growth.

In the third part of the table is found the comparison of ages. The first point to be noted is that the differences which have been found between hands in the first part and between the good and poor in the second part of the table are much less in the third part where ages are compared. Age differences count less than the other differences. The nine year old children in the class of good differ from the eight year old children

in the same class by 9.90, when the right hand is used and by 11.89 when the left hand is used, while the nine year old children in the poor class differ from the eight year old by 1.61 when the right hand is used and by .61 when the left hand is used. That is, the older children in the good class differ greatly and more from the younger than they do in the poor class. This, again, shows clearly the phenomenon of arrest.

The backwardness of these children may perhaps be better seen in the way in which each is affected by practice and by fatigue. Capacity for growing under practice, could it be readily and expeditiously tested, would seem to furnish the desired test of mental weakness. Indeed, practice effect is really the thing aimed at by educational processes. The ability of a child to take on new habits represents educability. The old and young differ most in just this respect. Backwardness may be treated simply as premature old age, or the failure in the natural plasticity that characterizes youth. The stupid child that cannot learn, possesses no capacity for being affected by practice.

While our tests do not furnish all that could be desired in this respect, they do cast some light upon the question. The five successive trials at tapping furnish opportunity for making some test of the capacity for practice effect. This may be done by computing the average trial gain or loss. The method employed here was to subtract the average for the first trial in order from averages for the subsequent trials, and then to treat each average in the same way for all subsequent trials. The differences thus obtained were added, and divided by the number of times the actual trial difference had been taken. The comparisons which appear in the following table are for the right and left hands of the good and poor without reference to age. The average trial practice gain or loss stands at the right.

TABLE II.

	1st.	2nd.	3rd.	4th.	5th.
Good. R. H.	29.60	30.29	30.52	30.70	31.20 = .361 gain
“ L. H.	25.57	25.41	26.59	26.30	26.49 = .269 gain
Poor. R. H.	26.85	27.35	27.25	27.40	26.78 = .09 loss
“ L. H.	23.75	23.66	23.78	23.87	23.58 = .11 loss

The right hands of the good children show an average trial practice gain of .361, and the left hands of .269; both the hands of the poor children show a very small average practice loss. That the left hands should not respond as quickly as the right is to be expected; but the fact that bright children show greater effects of practice is new and significant. It proves the fatigue-ability of the poor children. The children complained frequently of their arms being tired, in which case they were given an interval for rest, although the hands were alternated so that the one might rest while the other was at work. The feelings of fatigue, however, are often of central origin, and the brain worked in much the same way while either hand was doing the tapping. The extreme awkwardness of these children was most characteristic. Again, some of them were very suggestible. Inquiry was made about their food, whether they drank coffee and tea; and not infrequently they would answer both 'yes' and 'no,' according as the form of the question was changed from positive to negative. Some of them were so inattentive that it was with difficulty the observations could be made at all. This seemed to be due to fatigue; for seven or eight of them grew so tired that they asked to leave off the tapping after two or three trials. This could not have been due to mental disturbance, for they took up other tests. Another significant fact was that they tapped rhythmically. Many of them seemed able to make taps rapidly enough, but they could not execute more than eight or ten taps until they made a stop, or slowed up, and then went on with a fresh effort. Five seconds does not seem to be too long a time for a child to keep up a muscular effort but for these poor children it is plainly too long, and this indicates their weakness. We are inclined to believe that capacity for practice and fatigue-ability stand in an inverse ratio to one another. Practice increases the power to resist fatigue, and also accelerates the rate of recovery from it. In summing up, it can be said that capacity to grow through practice, and the power to resist fatigue, are accompaniments of intellectual brightness and form one test of mental strength.¹

¹ These facts are clearly brought out in unpublished work done in the Psychological Laboratory at the University of Nebraska.

Although the numbers of the children are rather small, we will nevertheless offer a comparison of the boys and girls with both the right and left hands. This will be found in the table following.

TABLE III.

Girls. Good. 9 yrs. R. H.	30.1	30.6	30.7	31.0	31.6
Boys. " " "	29.9	30.1	30.2	31.1	31.2
Difference	.2	.5	.5	-.1	.4 = 1.5
Girls. Good. 9 yrs. L. H.	26.8	27.8	27.8	27.0	27.1
Boys. " " "	26.1	25.8	27.1	26.4	26.5
Difference	.7	2.0	.7	.6	.6 = 4.6
Girls. Poor. 9 yrs. R. H.	28.5	28.5	28.3	28.4	29.0
Boys. " " "	27.0	28.	27.7	28.0	27.3
Difference	1.5	.5	.6	.4	1.7 = 4.7
Girls. Poor. 9 yrs. L. H.	25.2	23.9	24.7	25.1	24.9
Boys. " " "	24.1	24.6	24.0	23.9	24.0
Difference	1.1	-.7	.7	1.2	.9 = 3.2

These figures show that the girls are uniformly better than the boys. The girls in the good class do not show greater or even quite as much superiority over the boys of the same class, as the girls of the poor class show over the boys of the same class. The common opinion that backwardness or mental arrest takes a deeper hold upon boys than upon girls is, therefore, supported by a small margin, but it is too small to be given any emphasis.

We pass now to a consideration of the tests of precision and steadiness in movement. Four movements with either hand were made, and two trials with each hand for all four movements were allowed. These movements were either upward or downward in vertical direction, or toward the median plane or away from it in the horizontal direction. 'Toward the median plane' means a contraction of the arm, and 'away from it' an extension of the arm. The first is probably the more primitive and useful. The second, particularly with the right hand of right handed persons, is an acquired movement, much used in writing and drawing. We might expect that, since it is so frequently used, it would be the more precise and

steady; but this is by no means the case. Social customs have little power to modify fundamental movements that have been fixed by selection through heredity. Let these two movements be called respectively the inward and outward movements. The inward movement for both hands is always more steady than the outward. The difference between the inward and outward movements for the right hand is slightly less than for the left hand, showing that social customs may have affected slightly the precision of the outward movement through practice. This will appear in the table following. The figures represent the average number of times the needle came into contact with the metal in passing between the strips of brass. R. H. means right hand, and L. H. left hand.

TABLE IV.

Good children. 9 yrs.			
R. H. moving outward	18.04	L. H. moving outward	22.87
R. H. " inward	15.93	L. H. " inward	18.78
Difference	2.11	Difference	3.09
Good children. 8 yrs.			
R. H. moving outward	19.36	L. H. moving outward	27.21
R. H. " inward	17.71	L. H. " inward	19.93
Difference	1.65	Difference	7.28

When a child was asked to draw a straight line, or indicate how steadily he could move his hand, he invariably moved the hand outward. In this, of course, he follows social custom, and yet that does not mean the highest skill. This suggests that perhaps human beings possess biological possibilities of movement that we know nothing of, and that these will enormously increase our social efficiency when they have been discovered. Scarcely a day passes, now, that something new in this line is not brought to light.

We propose now to show that these tests prove much the same distinction between the good and the poor that the former test with the tapping apparatus has shown. In the following table a comparison will be made with respect to classes, good and poor, and with respect to age.

TABLE V.

Part I. Comparison of the good with the poor.

		RIGHT HAND.				LEFT HAND.			
		In.	Out.	Down.	Up.	In.	Out.	Down.	Up.
Good.	9 yrs.	15.93	18.04	19.59	24.91	18.78	22.87	24.50	28.93
Poor.	" "	25.15	27.23	28.00	36.92	30.08	30.69	33.69	37.23
Differences		9.22	8.19	8.41	12.01	11.30	7.82	8.19	8.30
Good.	8 yrs.	17.71	19.36	22.00	27.21	19.93	27.21	26.91	29.71
Poor.	" "	21.87	25.87	26.93	32.67	25.40	29.13	30.80	35.53
Differences		4.16	6.51	4.93	5.46	5.47	1.92	3.89	5.82

Part II. Comparison between the ages.

Good.	9 yrs.	15.93	18.04	19.59	24.91	18.78	22.87	24.50	28.93
"	8 "	17.71	19.36	22.00	27.21	19.93	27.21	26.91	29.71
Differences		1.78	1.32	2.41	2.30	1.15	4.34	2.41	.78
Poor.	9 yrs.	25.15	27.23	28.00	36.92	30.08	30.69	33.69	37.23
"	8 yrs.	21.87	25.87	26.93	32.67	25.40	29.13	30.80	35.53
Differences		3.28	1.36	1.07	4.25	4.68	1.46	2.89	1.70

In the first part of the table the figures designating differences represent the superiority of the good children over the poor. When these figures representing the difference between the good and poor are compared with the corresponding figures in the second part, representing the differences between the ages, it will be seen that they are much larger. It has already been shown that the good children stand on the average two grades in school work above the poor. The tests here would indicate an equally large difference in motor power. If now the difference between the good and poor at nine years of age be compared with the difference at eight years of age, it will be noticed that the nine year old children among the good show a greater superiority over the poor than they do at eight years of age. This was pointed out before and the difference now becomes still more apparent. This is best seen in the second part, where the comparison between ages is given. Nine year old children among the good are superior to the eight year old, as we should expect; but among the poor class the eight year old children are as much superior to the nine year old as the nine year old in the good class are superior to the eight year

old of the same class. The difference is simply reversed. This is to be accounted for in part by the fact that the nine year old children of the poor class represent those pupils in the public schools who have gone a number of years and have not been promoted. A nine year old child who has not made more than the second grade is to be accounted very backward. By that age the child begins to feel his defect, loses heart, and begins to think of leaving school. Such children are in the depression that precedes adolescent growth, and this falls with especial severity upon those that are poorly endowed. Among the eight year old children are some well endowed children, who, through accidents, are behind in their school work, and they help to bring up the general averages of the class. The school from which most of the poor children were taken contained only three grades, and most of these pupils were in the first two grades. The bright children were sent from here to other buildings after they reached the third grade. Those that did not get on rapidly were retained, and nominally advanced to a third grade, in which, for the most part, they did not do well.

A comparison of the boys and girls of nine years of age, upon the basis of this test, yields much the same result as was obtained in the tapping test. The figures are given in the following table.

TABLE VI.

	RIGHT HAND.				LEFT HAND.			
	In.	Out.	Down.	Up.	In.	Out.	Down.	Up.
Good. Boys 9 yrs.	22.9	22.7	19.2	19.5	25.3	24.8	24.0	22.3
“ Girls “	16.8	18.5	17.1	20.6	21.4	23.4	22.8	24.8
Difference	6.1	4.2	2.1	-1.1	3.9	1.4	1.2	-2.5
Poor. Boys 9 yrs.	20.8	23.2	23.1	28.3	24.5	27.0	28.0	31.2
“ Girls “	17.3	19.3	20.5	24.5	21.1	26.3	27.5	30.9
Difference	3.5	3.9	2.6	3.8	3.4	.7	.5	.3

The tests with the ataxiagraph were the least satisfactory of all. The instrument cannot be made with sufficient mechanical accuracy. The test, like that of the dynamometer, has a most general import. It is more severe upon the pupils in the way of a nervous strain; they do not see the meaning of it; and no interest can be aroused, either through suggestion or

through mutual rivalry. The results are appended without emphasis. The figures represent the amount of swaying of the body in mm. in the anterior-posterior and lateral directions.

TABLE. VII.

	Ant. Post.	Lateral.
Good	16.2 mm.	11.7 mm
Poor	18.8 "	12.4 "
	<hr/> 2.6 "	<hr/> .7 "

This indicates a small difference in favor of the good. A number of children that were plainly weak were tested, and they swayed more in the lateral than in the anterior-posterior direction. This has been pointed out before as a symptom of nervous weakness. The movement in the anterior-posterior direction was very generally forward. The amount of movement was measured by laying two carpenter's squares together in such a way as to form a quadrilateral that would enclose all the markings of the writing point. The distances were then read off directly upon the corners of the squares. This takes no account of the actual amount of swaying, only of the extreme limits within which swaying took place.

We must decide against the ataxiagraph, as an instrument that is unadapted for finer psycho-physical measurements. The tapping test and the test of precision and steadiness in moving seem to us, when made with care and patience, to yield fairly trustworthy results. A number of observations upon each pupil, under skillful management, is always required and the outcome will be indicated not so much by the absolute value of the figures obtained as by the indication they give of gain through practice or of the resistance offered to fatigue. The curve of practice must first be determined, before the value of any observation can be determined, and influences must be studied by the way in which they affect the curve of practice.

These tests have shown that with the brighter children motor power increases with advancing age. There is greater rapidity of motion, increased steadiness and nicer precision, the older the children grow. Backwardness, slowness of growth and arrest of development are indicated by pupils through their

inferiority to their fellows of the same age in some or all these respects. The explanation of motor development is based upon the growth of interrelations among nerve elements. Cells put out processes, which extend sometimes considerable distances in all directions throughout the nervous system. These processes place the cells in communication with many of their neighbors, so that when they are thrown into activity their neighbors must act; and many cells or groups of cells acting simultaneously make possible precise, rapid, and nicely adjusted movements. The number of cells does not change, probably, after foetal life, subsequent change being due always to increasing connections between isolated ganglia. Arrest of growth is thus confined entirely to a suspension in the growth of associative connections.

The relation of mental to motor development finds its explanation in something like this: The movements of the voluntary muscles are felt in consciousness; in fact, the possibility of a voluntary movement depends upon the consequences of the movement being felt. The greater the variety of movements that can be performed, the more precise they are, the more steady and rapid, the greater the fund of sense experience they will yield up to consciousness, out of which are to be built the various products of mental activity. Every new movement acquired adds a new piece of furniture to the mental household. Movement may not be the sole source of mental representations, but representations of movements do enter into our mental constitutions, so that the higher our motor development has progressed, the more will our consciousness be built up from this source.

Mind, whatever its metaphysical nature may be, is a device to aid us in getting on in the world of things; minds are to direct activity and to control conduct. Our organisms are so constructed that impressions made upon the sense organs tend to issue in motions. The purpose of sensation is, then, that movement may be directed. These movements are protective and preservative of the organism, and it is thus for the sake of movement—conduct in the large sense—that mind exists. Accordingly mind and movement must develop together; for without movement there is no mind. In so far forth as an individ-

ual is wanting in motor development, he is wanting in mental development. The aim of all instruction for the feeble minded is to awaken movement; when that has been accomplished, mental development will take care of itself. This has an important bearing upon the beginning of educational work. Nature has pointed out the way, by filling the young of higher animals with impulses to act, following which they discover shortly in their plays most of their possibilities for acting. Much has been accomplished, when it has been shown that lack of mental alertness and intellectual brightness is accompanied by a deficiency in motor power; and the goal will have been reached when accurate tests have been found, which shall indicate the degree of this deficiency. The further problem is a practical one, first, to find methods of awakening and developing motor power, and second, where the deficiency is proved to be permanent, to find an education that is suited to the needs of the defective.

ARE CHROMÆSTHESIAS VARIABLE?

A STUDY OF AN INDIVIDUAL CASE.¹

By Professor F. B. DRESSLAR, University of California.

In the spring of 1895, while making some psychological experiments with a class of normal school students, it became evident to me that one member of the class, an intelligent young woman, had in some way developed a large number of color associations. Upon questioning her carefully, so as to avoid as far as could be all possibility of suggestion, I also found that she had clearly marked 'forms' for number series, for days of the week, months of the year, hours of the day, in fact for all associated groups of figures, letters, or names. She was as much interested in finding that the other members of the class did not image as she did, as they were in learning of the mental imagery which she employed.

After some preliminary study of her subjective color sensations, it occurred to me to institute a series of tests to determine what changes, if any, her associations of this kind would undergo. It was plain in the beginning that the experiments would have to cover a number of years, and that a considerable space of time would have to elapse between each test in order to eliminate the element of memory. It was found, too, after beginning the work, that the tests to be of real value must be of short duration; that is, the element of mental fatigue must not be introduced. It was noticeable that, in order to make any careful estimate or judgment of the exact color sensation attendant upon a given image, the subject was compelled to discriminate very carefully and at the expense of a good deal of mental energy. For example, it was easy for her to decide at once that a given name was "reddish," but when she was

¹ Much good work has been done upon chromæsthesia and kindred topics in the last few years, but not so much, the writer trusts, as to make the record of repeated tests at long intervals upon a single subject without interest.

pressed to describe in detail, notwithstanding objective standards were at times furnished, it seemed to impose upon her a severe mental strain, and one which, if persisted in for any considerable length of time, would introduce the disturbing element of fatigue.

After some preliminary tests, the work began with observations on a list of common Christian names. These were selected because it was found that the color feeling associated with them seemed quite marked, and in a way, influential. That is to say, there seemed to be a personal element in names which was lacking in the case of other words; and this had apparently helped to make the associations clear. This last statement, however, must be understood as in no sense representing the result of serious investigation. It represents a conviction arrived at during the preliminary tests. It would have introduced into the work a great many difficulties in the way of suggestions, if any other plan had been pursued. Later results showed the necessity of this precaution. And it may not be out of place to say that throughout the whole investigation much care has been taken to prevent suggestion.

The following list of names was taken for the first tests, and, in order to have some generally accessible standard with which to compare impressions, the colors given on page 1723, Vol. II, of the Standard Dictionary were taken. As will be seen, however, these were insufficient in variety of colors, tints, and shades, and in a few cases other standards were introduced.

The method used in these first tests consisted in announcing the names to the subject while she found an objective color to match that associated with the name. In these tests she saw neither the whole list of names nor the records of the matchings made. For each of the three tests here tabulated the names were presented in a different order, and in no case alphabetically. They have been here uniformly arranged, merely to make the comparison of results easier.

Records 4 and 5 belong, chronologically, to the series arranged above, as has been indicated; but owing to the fact that the color chart in the dictionary was not taken as a standard in either of these tests, it seems better to tabulate the results separately, while the words are arranged in the same order.

	FIRST RECORD, JULY 2, 1895.	SECOND RECORD, AUG. 3, 1895.	THIRD RECORD, AUG. 10, 1895.	* SIXTH RECORD, MAR. 11, 1903. * See below for fourth and fifth series.
ANNIE,	(See Standard Dictionary Color Chart, Vol. II, p. 1723.) Melon.	Melon with a single tissue paper over it.	Orange with double tissue over it.	Orange with double tissue. "It seems uncertain."
CECILIA,	(Indefinite.)	(Very little color.)	About the same as the margin of the leaf of the Dictionary.	
CHARLES,	Straw with tissue† over it. "It is a dull shade." († Always the tissue leaf found in the Dictionary at this place.)	Straw with tissue.	Canary with tissue.	Straw with double tissue over it.
CHARLIE,	Light yellow with tissue.	Straw with thin tissue. "Lighter than Charles."	Straw with tissue.	Straw with double tissue, "but much lighter."
CORNELIA,	(Not clear.)	(Indefinite.)	Lemon with a thick and a thin bit of tissue over it.	Straw with single tissue over it.
DORA,	Cherry with tissue over it.	Pink with thin tissue over it.	Clear melon with tissue over it.	Pink with tissue over it.
EDITH,	Light bluish gray. (Not on Chart.)	Bluish gray with tissue over it.	Light bluish gray.	Light bluish gray, "Nothing like it in the Dictionary."
EDNA,	Light bluish gray.	Bluish gray with tissue over it.	Light bluish gray.	Turquoise with double tissue over it. "But lighter, with more gray in it."

ELIZABETH,	Bluish gray, Pacific.	(Unable to decide.)	(Indefinite.)	Robin's-egg blue with double tissue, "but needs a little more gray."
EMMA,	Light yellowish white.	Light yellowish.	Margin of the leaf of the Dictionary with tissue over it.	Straw with double tissue over it, "but should be lighter."
ESTHER,	(Unable to decide.) "Seems indefinite."	(Indefinite.)	Salmon pink with double tissue over it. "It grew redder."	Salmon pink with double tissue over it.
GERTRUDE,	(No decision.)	Y of the normal spectrum with light tissue.	Y of the normal spectrum with double tissue over it.	Lemon with double tissue. "Better if the color were in liquid."
GODFREY,	O in the lowest strip of the color spectrum with tissue over it. "It has a reddish cast."	Melon with tissue over it.	Salmon pink with tissue over it.	Y O of the lowest strip of the color spectrum.
HELEN,	Bluish gray. "More bluish than Elizabeth."	Bluish gray "when in a darkened room."	Bluish gray. "About the color of the cover of the Eclectic Magazine when in a shadow."	Turquoise. "But it ought to be duller and have more blue in it."
HENRY,	(No decision.)	(No record taken.)	Dull gray, tinted with blue.	Gray with tissue over it. "Lower parts of E and H lighter."
JACK,	Black. "Color of the letters when made with printers' ink."	(No record taken.)	Black "like printers' ink."	Gray with tissue. "But needs more black in it so as to look like black ink."
JAMES,	(No record.)	"James does not feel active to-day and hence do not want to decide on it."	O of the normal spectrum with light tissue.	Brown with double tissue.

JANE,	(No record.)	(No record.)	"It is the color of a light roan horse and a dismal color." (Nothing like it in the dictionary.)	Apple green with double tissue. "But needs to be a little darker."
JESSIE,	Pink with tissue over it.	(No record.)	Pink with double tissue.	Scarlet with double tissue.
JOHN,	"Yellowish brown."	Yellowish brown with light tissue.	Yellowish brown with light tissue.	Coffee with single tissue over it. "But needs more brown."
JULIA,	(No record.)	(No record.)	Vg of the lower line of spectrum colors. "Yellowish green, like sage."	Olive green with single tissue. "But lighter and more like sage."
MARY,	"Indistinct shade of red."	Terra cotta with tissue over it.	Red I, with tissue over it.	Rose with double tissue. "It would be better were it duller."
MAUD,	"Like lustrous black morocco leather."	Gobelin blue with tissue over it.	Gobelin blue with tissue. "But a little blacker."	Olive green with double tissue. "But needs to be blacker."
MINNIE,	Nile green with tissue over it.	Blue with double tissue over it. "But should be darker."	Antwerp blue with tissue over it.	Gobelin blue with double tissue over it.
ORIS,	(No record.)	Salmon	"Light yellow scratch paper, with tissue paper held a little above it."	Straw with double tissue over it. "But some lighter, and the last part containing a shade of red due to the S."
ORRO,	"White of a yellowish cast." (Not in the dictionary.)	(No record.)	(No record.)	Straw with double tissue over it.

SAMUEL,	(No record.)	"Not clear but somewhat reddish."	II in the normal spectrum, in faint side of band with tissue placed over it.	Salmon pink with double tissue over it.
SARAH,	Dull faded terra cotta. "But lighter than that shown in the dictionary."	Scarlet with tissue above it.	(No record.)	"Like ordinary black ink."
THOMAS,	(No record.)	(No record.)	Nile green with double tissue over it.	

Records 4 and 5 represent the attempt of Miss S. to match her subjective color sensations for the names with an objective series of colors, which she herself painted, pasted, or pencilled. Record 4 was made in the following way. The list of names, irregularly arranged on the left margin of the page, was furnished her. She then constructed opposite each name and in the middle of the page, a band of color matching her color association for the name. Record 5 was taken nearly three years later, and in the following way: The names on the left margin being concealed, she was asked to write, on the right margin opposite each color band, a name that would correspond in color to it. No list of names was furnished her for record 5, and hence any possibility of remembering her former matchings was reduced to a minimum.

It would have been better if these color representations could have been reproduced here just as she made them, but as this seemed a practical impossibility, it was thought best to have the matchings made by one wholly unacquainted with the tests, and in this way to be able to approximate the colors as originally made. In pursuance of this plan, Dr. F., a man skilled in color discrimination, was asked to take the original colors and match them with the colors in the dictionary. The reader can, therefore, by following the directions given in the spaces originally occupied by the color bands, reproduce very nearly the colors that were made by Miss S.

Immediately after record 4 was made Miss S. undertook to describe her introspection for these name colors. Herewith I append a part of this description, for the sake of its bearing on the whole experiment :—" I see the names in just about the colors indicated. When I think of the name Edith, for example, I see it as it appears written, and it is colored a bluish gray. The colors are all soft, as if seen in a mellow evening light. It seems impossible to show by colors, or express in words, the exact truth of the coloring. I see the name Gertrude as it appears written. The letters are pale yellow with a little tracing of brown. Jessie is a mixture of light with some shade of red, but it is not decided. It is very much like the color of this stamp just above the word farming. (Here was inserted a two cent postage stamp of the Buffalo Fair va-

riety.) The names, except Gertrude, do not appear as if written in colored ink. While I see them as they look written, the color is around them and seems a part of them. They are almost like objects."

From a study of these records it will be seen that there are several interchanges of these names, but in no case do the colors assigned to any one name differ materially. Taking into consideration the whole run of the experiments, and also the subject's analysis of her own consciousness, there seems to be no doubt of the fact that the variations here shown are due to inability to make exact matchings, rather than to any change in the subjective color sensations. For example, the color band that was made to suit her association for the name Edith, in test 4, is judged in test 5 to represent Helen. But it will be seen that the difference in color for these names has in all tests been very inconspicuous, and consists simply in a slightly differing shade of gray. The same general fact will be noticed in the other interchanges.

RECORD 4. Sept. 16, '98	MATCHINGS OF COLORS IN THE DICTION- ARY WITH THOSE CORRESPONDING TO NAMES SET OPPOSITE. (See explanation in the text.)	RECORD 5. July 23, '01.
EDITH,	Dove—with a single tissue over it; but a little bluer and lighter.	HELEN.
EDNA,	Gray—with a single tissue over it; but duller.	EDITH.
DORA,	Cherry—with single tissue over it.	DORA.
ANNIE,	Red I—with single tissue over it.	ANNIE or SAMUEL.
MINNIE,	Gobelin blue—but a little lighter.	MINNIE.
OTTO,	White margin of Dictionary; but a little more yellow.	CECILIA.
GODFREY,	Melon—with a single tissue over it; but a shade darker.	SARAH.
MAUD,	Gray—but a little warmer.	MAUD.
HENRY,	Coffee—but lighter and not so solid.	JAMES.
JAMES,	Café-au-lait (very nearly).	JANE or JULIA.
JOHN,	Brown—but less massive.	JULIA.

SAMUEL,	Salmon pink—with single tissue, but a pink.	SAMUEL.
THOMAS,	Dove — with single tissue, but not so solid.	THOMAS.
MARY,	Cardinal— single tissue over it.	DORA. (Not quite.)
CHARLIE,	Fawn—with single tissue, but paler.	CHARLES.
HELEN,	Turquoise—single tissue, but a little paler.	HELEN.
EMMA,	The color of the margin of the Dictionary.	OTTO.
JACK,	The color of good black ink.	JACK.
CHARLES,	Straw—with single tissue.	CHARLES.
CECILIA,	Margin of the Dictionary, but whiter.	OTTO.
CLAUD,	Margin of the Dictionary, but more yellow.	EMMA.
CORNELIA,	Pearl gray—with single tissue, but a little lighter.	"Not active."
JULIA,	Ochre—but greener.	JULIA or JANE.
ELIZABETH,	Turquoise— with single tissue over it.	EDITH. (Not so dark.)
ZORA,	Gobelin blue—but a little lighter and less blue.	MAUD.
LIZZIE,	At BBG, in the third line of spectrum colors.	LIZZIE.
SARAH,	At ORO, in the third line of the spectrum colors.	DORA or SAMUEL.

COLOR ASSOCIATIONS FOR LETTERS.

Early in the experiment, the hint came that the color sensations connected with names, or with words in general, were closely related with the colors associated to the letters composing the words. A month later than the first of the three tests tabulated above, a record of the associations for the different letters of the alphabet was taken, in the same manner as that described for words. The letters were not given in their order, as arranged here, but were mixed up indiscriminately. They have been arranged in their customary order simply for convenience.

	FIRST RECORD, AUG. 1895. Dictionary chart used as standard.	SECOND RECORD, SEPT. 1898. Descriptive. No objective standard used.	THIRD RECORD, JULY, 1901. Descriptive. No objective standard used.	FOURTH RECORD, APRIL 13, 1903. Dictionary chart used as standard.
A.	Pink—with tissue over it. "About the color of Annie."	"Reddish."	"Light color."	Salmon pink with dou- ble tissue over it; "but a little duller."
B.	Light blue. (Probably an error.)	"Yellowish brown."	"Blackish."	"Like ordinary black ink;" but lighter.
C.	"White."	"Cream."	"Pale cream."	"Light cream color."
D.	"Dark—a <i>very</i> dark brown."	"Brownish black."	"Dark."	Brown with double tissue. "But needs more black."
E.	Bluish gray, "like Eliz- abeth."	Grayish blue; "like Ed- na and Edith."	"Light gray."	"Very light bluish gray."
F.	"Dark, like printer's ink."	"Black."	Black—"has a denser appearance than T."	Between a "brownish black" and Gobelin blue with a single tissue over it.
G.	I—in normal spectrum.	"Yellowish red like Godfrey."	"Yellowish red."	Drab—with single tissue.
H.	Ecrû—"with heavy tis- sue over it."	"Color of the name Hen- ry."	(No record.)	"Like ordinary black ink."
I.	"Dark like ink."	"Dark like ink."	"Black, but not dead black."	"About the color of black ink."
J.	"Dirty greenish yellow, something like the color of Julia."	"When I first think of J, it is something like the color of sage; but it soon becomes black like ink."	"Dull black."	"Brownish, with dark sage green cast."

K.	Sapphire—with heavy tissue over it. "It is dark with a little blue. It is related to Kate."	"Deep gray blue."	(No record.)	"Like blue-black ink."
L.	"Printer's ink, but immediately verges toward the color of Lizzie, and yet it is not quite so bright as that name."	"Bluish black."	"Bluish."	"Bluish black ink."
M.	"About the same color as printer's black ink."	"Dark like ink."	"Bluish black."	Gobelin blue with double tissue over it. "A little duller."
N.	"Black like printer's ink."	"Dark."	"Dark like ink."	"Brownish black ink."
O.	"White with a very light yellow tinge."	"White."	"Light."	"About the color of waiter."
P.	Nile green—with heavy tissue over it. "And then a little green added."	"Something of a blue."	"Dark; not clear to me."	Peacock-blue with double tissue over it.
Q.	Between 9 and Fawn, "but nearer 9."	"Greenish yellow."	"Soft tan; but am conscious that this is not a good description."	Lemon with double tissue; "but duller."
R.	"Black-brown—with a slight reddish cast."	"Brown."	"Blackish but not like B."	"Brownish-black ink."
S.	Orange — with tissue. "Pretty red."	"Red — something like Dora and Samuel."	"Red — something like Samuel."	Salmon pink with double tissue over it.
T.	"Dark like heavy print."	"Black."	"Black like Thomas."	"Like black ink."
U.	"Color not clear."	"Something like W."	"Brownish."	

V.	Cyan-blue, with heavy tissue over it. "This is still a little too light."	"Dark."	"Something like W."	Gobelin blue with single tissue over it. "But a little blacker."
W.	"It has no definite color, but gives me a sort of liquid impression, as if produced by an easily flowing sound."	"Like watery ink."	"Watery black."	
X.	"Dark—no particular color. Somewhat like print on a page."	"Dark."	"Dark."	"The color of brownish black ink."
Y.	Fawn—with heavy tissue over it. "Not quite so decided."	"Dark like ink."	"Some dull color with mixture of dull yellow."	Ochre with double tissue over it. "But darker."
Z.	Lustrous black with a cover of heavy tissue. "Much like the dark stripes on a zebra."	"Color of a zebra."	"Changing from dark to light."	Gobelin blue with a single tissue over it.
&.	"Very light bluish gray; fresh looking."	"Light, almost white."	(No record.)	

The color charts in the Standard Dictionary did not, as in the case of words, furnish sufficient variety in colors, shades, and tints, to match all the letters. In these cases other means have been employed to describe the associations.

Near the close of the work I prepared the following list of questions, with a view to getting whatever help or caution the answers to them might disclose or suggest.

QUESTIONS.

1. What is the difference in your feeling, if any, in looking at a real color and thinking of a name with the same color?

Answer. A name color seems to be nearer to me; it seems more a part of me than the real outside color.

2. Do you see a name in color when you look at it as it appears written on a page with ink or pencil? *Answer.* No. It is an internal feeling, and is purely imaginary.

3. Do these associations influence your preference for names? *Answer.* They do. I do not like those names associated with the reds. I like bluish names. On the whole, however, my love for a name grows with love of the person bearing it.

4. Is there an æsthetic desire to see a person wear clothing of a color that will go well with the name color of that person? *Answer.* No. But there is a desire to see the name color and the complexion harmonize.

5. Does the color of a name influence your feeling toward the person bearing that name? *Answer.* No, not that I am conscious of.

6. Do these subjective color sensations come out brighter when your eyes are shut? *Answer.* Yes. I really seem to see the colors better; but I feel that the difference is due to the removal of other stimuli. It helps to get the light out of my eyes. Then it is necessary to concentrate my attention to get good color feeling in certain cases, and I can concentrate better when all conflicting stimuli are put aside.

7. Have you any regular associations coming up along with those of color? *Answer.* I have not.

8. Are you conscious of being troubled by these color associations at any time? *Answer.* They do not trouble me.

On the other hand they have been, and now are, a source of great pleasure and much help.

9. When you think of a name does it seem to occupy any special place or direction? *Answer.* I see most names about an arm's length in front of me. I see Samuel out in the air and in the direction in which he lived. When I think of the name Godfrey it is to my right and a little further away than Samuel.

10. Are you conscious of the names being a mixture of the colors of the letters composing it? *Answer.* I am very conscious that this is the case.

11. Does the position of a letter in a name give it special influence in shaping the color of a name? *Answer.* When the initial is a letter which has a decided color, it tends to color the whole name. Otherwise the letter of the most decided color will influence most.

12. Are you conscious of any changes taking place in your color associations for names or letters? *Answer.* The color of *s* seems to be changeable.

13. Do you know which first came to be associated with color—names, or letters? *Answer.* I do not.

14. Do the letters composing a word, or the digits making up a larger number, always fuse into a solid color, or do their colors remain dissociated to any degree? *Answer.* They do not always fuse into a solid color. [It will be seen that the solidity of the color depends on the dominating influence of some one or more strongly colored letters making up the word.]

15. Is your color association for a word the same when you call it up in memory as when you look at the word written or printed? *Answer.* No; the color is more marked when I merely think of a name, than it is when I see it written or printed.

16. Are you conscious of color associations with all kinds of words? *Answer.* I am.

17. Have you any color association for tones when not connected with words? *Answer.* I have a keen perception of difference in tones, but have no colors to correspond.

18. Is the color of a word influenced in any way by the quality of the tone used in speaking? *Answer.* It seems to

be. [I found by experiment that at times a harshly spoken word produced what the subject described as a "ragged appearance of the letters and colors;" while the same word spoken in a mild quiet voice had no such effect. In the latter case not only did the letters seem less irregular, but they were also inclined to take the script form in a smooth flowing hand.]

GENERAL SUMMARY.

1. During a period of time covering nearly eighty years, there have been no appreciable changes in the color feelings of this subject for the letters of the alphabet or for the names used in the tests.

2. The color of a name results from the mixing of the colors associated with the letters composing the name. But it will be observed that the initial letter and those having striking color characteristics dominate almost to the exclusion of those with weaker colors.

3. These color feelings are most pronounced when the nervous system is least fatigued, and when all objective stimulations are removed.

4. The subject of these experiments has experienced no inconvenience, so far as she can tell, from this striking mental tendency. On the other hand she thinks it has been of service to her.

5. These feelings are probably due to some form of suggestion, or direct perception, occurring in childhood, and have become fixed by habit.

ON THE GUESSING OF NUMBERS.

By Professor E. C. SANFORD, Clark University.

The psychology of Belief has received some attention from psychological writers, but the psychology of Guessing—the formation, in the absence of adequate data, of estimates and opinions about the ordinary affairs of life—has not often been considered. A thorough-going study of it might, however, be expected to throw light upon some of the less obvious, and perhaps unconscious influences, that determine opinion and action. The discussion which follows is a fragment of such a study with reference to a sort of guessing of which instances are particularly easy to obtain in quantity, the guessing of numbers in “Guessing Contests.”

This field is not wholly a new one. Professor F. B. Dresslar has contributed to the *Popular Science Monthly* (Vol. LIV, 1898-99, pp. 781-786), a study on “Guessing, as Influenced by Number Preferences,” based upon the guesses recorded in a “guessing contest” upon the number of seeds contained in a monster squash. Professor C. S. Minot reports in an early number of the *Proceedings of the American Society for Psychological Research* (Vol. I, 1885-89, pp. 86-95), an investigation of “Number Habit,” which, though making use of material from quite a different source, bears upon the same general question. Still others have written with reference to number habit or number preference as these appear in the census returns and in judicial sentences. To these special reference will be made below.

The material for the present study was derived from a “guessing contest” conducted for advertising purposes by a Worcester dealer in photographic supplies, the prize being a valuable camera. The guesses were upon the number of beans in a “five pint” bottle filled to the cork with small white beans and conspicuously displayed in the show window. Customers were given with their purchases cards with places

marked for the inscription of a number and for a name and address. These cards were filled out at the time or later, and deposited in a box conveniently placed for the purpose. The cards deposited furnish the statistical record for the following study.¹ On the cards appear the names of 765 persons, 651 men and boys, 114 women and girls; and 10 cards were deposited without name. The total number of cards coming into my hands was 2,817. The guesses range from 285 to 3,425,602 for the men and boys, and from 250 to 2,675,181,756 for the women and girls. Guesses of 1,000,000 or over are few in number, and some, if not all, were probably set down in sport. Of the total number of guesses 2,573 were made by men and boys, 244 by women and girls. The actual number of beans, as reported in a current newspaper item on the award, was 8,834, and the winner a man.² The vast majority of the contestants guessed but once or twice, but a few guessed as frequently as 30 or 40, and two, more than 50 times each.

As it seemed likely that the conditions under which repeated guesses would be made might be different from those of the casual guesser, the cards were separated into two groups, one consisting of the guesses of those whose names appeared not more than five times, the other of those who guessed six times or more. Later the guesses of the women and girls were removed from both groups for separate consideration, making three groups in all. The following study covers the first and last of these groups; the frequent guessers for the present have been left out of account.

The group of infrequent guessers consisted of 535 persons (men and boys), who, with the anonymous guessers, deposited a total of 1,050 cards, an average of not quite two cards apiece. It is fair to suppose that we have in this group a set of guesses practically uninfluenced by considerations outside those involved in the simple guessing at the number of beans, and so large that individual tendencies will disappear in the mass. The group is thus fitted to show general tendencies, if any such exist.

¹ My thanks are due to Mr. Langdon B. Wheaton, for kindly placing this material at the disposal of the Psychological Department of Clark University.

² *Worcester Evening Gazette*, Jan. 1, 1901, p. 1.

Of the original 1,050 cards two were removed because of illegibility, and one group of five from a single guesser were thrown out because the numbers showed signs of playful choice. The following relations have been worked out on the basis of the 1,043 cards remaining.

The range of guesses here was 285 to 1,000,000. The median guess (the middle one when the 1,043 guesses were arranged in order of size), was 7,257, over 1,500 short of the actual number if the newspaper figures were correct. The medians of the upper and lower halves of the series, which give the limits within which falls, as nearly as may be, one-half the total number of guesses, are 4,173 and 9,536. The range from 1,200 to 16,000 includes a little short of nine-tenths of the guesses. The following table shows the distribution in the several thousands up to twenty thousand.

TABLE I.

Distribution of 1,043 guesses according to the thousands in which they fall.¹

0 — 999	31	7,000 — 7,999	142	14,000 — 14,999	10
1,000 — 1,999	86	8,000 — 8,999	113	15,000 — 15,999	11
2,000 — 2,999	78	9,000 — 9,999	100	16,000 — 16,999	5
3,000 — 3,999	55	10,000 — 10,999	57	17,000 — 17,999	10
4,000 — 4,999	76	11,000 — 11,999	34	18,000 — 18,999	3
5,000 — 5,999	71	12,000 — 12,999	23	19,000 — 19,999	6
6,000 — 6,999	87	13,000 — 13,999	10	20,000 and over	35

In this table there is a massing of the guesses between 7,000 and 10,000, but also a disproportionate number falling between 1,000 and 3,000. The first is no doubt the result of a genuine effort to estimate the number of beans in the bottle—an estimate in units of a thousand is, under the circumstances, not at all unreasonable. The second is probably not due to such an effort, nor yet to a wide-spread preference for the digits 1 and 2,

¹ The grouping used in this table is, strictly speaking, not quite as stated in the heading. The first thousand should include the figure following 999, and the second that following 1,999 and so on, but it is safe to say that in the minds of most guessers the change to a new numerical species occurs when the digit appears or is changed in the thousand's place. Witness the popular confusion as to whether the 20th century began with New Year's day 1900 or 1901.

but rather, it would seem, to the fact that many careless or indolent guessers used the numbers 1,000 and 2,000 (and the other numbers falling in these thousands) to indicate indefinitely "some large number." This would agree also with the fact that a large proportion of round numbers appear among the guesses falling within these limits. (*Cf.* tables in the section on Round Numbers below.)

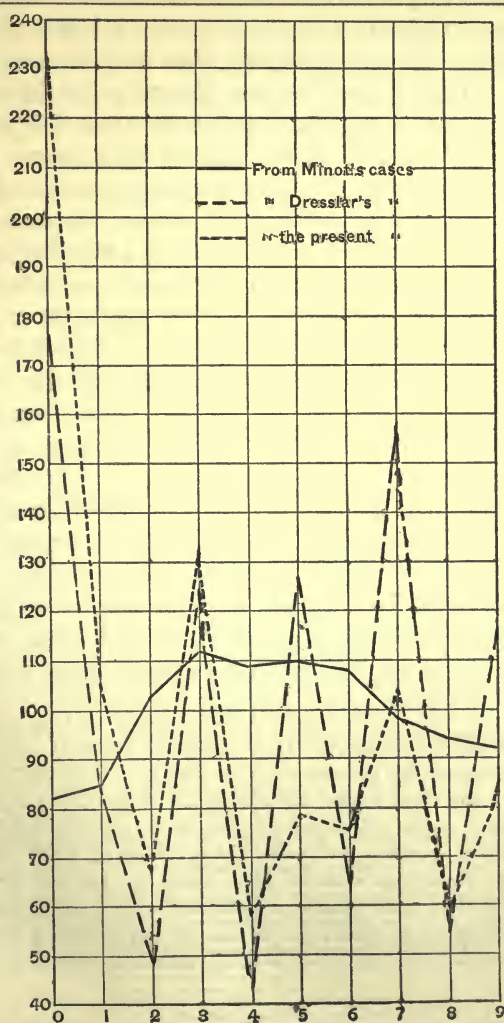
If the digits in the thousands' place are determined, in so far as they are not merely careless, by a bona-fide estimate of the number of beans, the digits in the tens' place and still more those in the units' place must be looked upon as determined by subjective considerations,—that is, as pure guesses. It is in the frequency of the recurrence of different digits in these places, therefore, that we may fairly look for number habits or number preferences. Here, also, it is possible to compare our results with those of Minot and Dresslar. Dresslar's statistics were derived, as I have said, from guesses made at the number of seeds in an uncut squash; Minot's came from the tables of digits set down by "percipients" in certain tests of "thought transference." The conditions of the guessing were thus similar in the three studies but not identical. Minot's guessers were confined to the first nine digits and zero (or ten). Dresslar's, though there was no limit set, fixed for the most part, upon numbers lying below 1,000. In Minot's cases there was absolutely no objective suggestion of the number to be guessed, and in Dresslar's only such as might arise from recollection of the appearance of other squashes when cut. In the present case, however, the beans could be seen distinctly through the glass of the bottle and there was a definite suggestion of multitude. Minot's material was gathered from the guesses of ten persons, Dresslar's and my own from the combined guesses of many individuals. The tables of Minot and Dresslar rest upon much larger masses of data than mine, Dresslar's covering 7,700 guesses and Minot's 8,600. My 1,043 instances are sufficient, however, for such comparisons as I shall institute.

The relative frequency of the digits as they appear in Minot's material and in the units' place in Dresslar's and mine is shown in the following table and chart. The figures in the table are all reduced to a thousand basis for ease of comparison.

TABLE II.

Frequency per thousand guesses of the various digits when guessed singly, or when set in the units' place in larger numbers.

DIGITS.	0	1	2	3	4	5	6	7	8	9
Minot's Cases,	83	85	103	112	109	110	108	98	94	92
Dresslar's Cases,	179	84	49	125	43	128	64	156	54	116
Present Cases,	231	107	67	132	58	79	75	105	59	85



It will be observed at once that the frequency curve given by Minot's cases is strikingly different from the other two. The variations from digit to digit are not extremely great; there is a gradual rise from the beginning to the middle numbers of the series, followed by a gradual decline in the later ones; and there is practically no difference between odd and even numbers. The other two curves agree in direction throughout, and, except for the greater frequency of the fives, sevens and nines in Dresslar's curve, may be regarded as essentially parallel. They agree in showing a high frequency for zero and for the odd numbers, with an equally marked deficiency in the even numbers. The difference between the curve for Minot's cases and the other two comes undoubtedly from the difference in the conditions under which the guessing took place. Minot's guessers were limited to the first ten digits and tended somewhat to avoid the numbers at either end of the scale. Traces of this tendency, though appearing less uniformly, are to be found in the records of most of his ten subjects. (See his table giving the individual records, p. 90 of the paper cited.) Subjects required to choose numbers within such restricted limits probably tend, as they make guess after guess, to pass irregularly up and down the series, and this brings them twice across the middle region for one arrival at either end, and so increases the probability of guesses from that region.¹ For this reason, also, Minot's material is not very well suited to bring out general number preferences, if any such exist, for they are cut across and obscured by the movements up and down the scale. The smaller number of guesses per individual, and the greater range of guessing allowed by the conditions under which the data for the other two studies were gathered, freed the guessers

¹ Prof. Minot does not consider this bunching of the guesses upon the mid numbers of the series. He discovered, however, a very marked tendency in one of his guessers to move with considerable regularity up and down the digit scale. He also tried to determine by statistical means whether or not the other subjects had a similar habit, but with largely negative results. In spite of this, however, I am inclined, for reasons that will appear later in this paper (*cf.* the section on Serial Numbers) and from the results of informal tests made some years ago with the writing of numbers up to 100, to believe that the habit of guessing up and down the scale is by no means uncommon.

from these tendencies, but opened the way for others. One of these is guessing in round numbers. It is this which accounts for the prodigious excess in numbers ending in zero—guesses in even thousands, hundreds, and tens, all combining to swell the total.

Round Numbers. To estimate in round numbers is the natural way of dealing with anything that cannot easily be made definite. It is the way also that would appeal most strongly, in a "contest" of this sort, to indolent or careless guessers.

A similar tendency has been found to affect the tables of ages in the census returns and the lengths of judicial sentences, increasing notably the frequency of ages and sentences that end in zero or five. Williams, for example, gives tables for the range of ages from twenty-eight to forty-two for the states of Alabama, Michigan, and the whole United States, based on the census of 1880, of which the following table is a condensation.¹ The unit in the table is 1,000.

AGE.	ALA.	MICH.	U. S.	AGE.	ALA.	MICH.	U. S.
28	19.2	30.0	850.0	36	10.5	21.8	581.6
29	11.2	23.1	621.8	37	8.7	19.2	495.1
30	30.9	32.5	1,094.3	38	11.3	21.3	594.5
31	8.4	18.9	492.5	39	7.3	17.7	458.0
32	12.4	24.4	654.8	40	23.2	26.0	922.6
33	10.6	21.9	580.9	41	4.6	12.6	323.6
34	10.0	21.0	546.2	42	6.8	17.5	458.9
35	22.3	26.3	871.0				

The same sort of thing appears in some degree in the corresponding tables of the censuses of 1890 and 1900, in spite of definite efforts to lessen or exclude it.²

The following table of judicial sentences is given by Wines in his pamphlet upon prison statistics in the census of 1880.³

¹ Williams: *Favorite Numbers*, *Scientific American Supplement*, Vol XXVII, 1889, pp. 11,008-11,009.

² See the discussion of this matter in the Report of the *Twelfth Census of the United States*, 1900, Vol. II, pp. xxxv ff.

³ Wines: *American Prisons in the Tenth United States Census*, New York and London, G. P. Putnam's Sons, 1888, pp. 24-26. Havelock Ellis gives similar data for English prisons in his work on "The Criminal," and Hewes gives a popular account of data gathered from the census of 1890 in *Harper's Weekly*, Vol. XL, 1896, March 14, p. 254.

SENTENCE.	NO. OF CASES.	SENTENCE.	NO. OF CASES.	SENTENCE.	NO. OF CASES.
1 year,	3,647	21 years,	120	45 years,	5
2 years,	6,028	22 "	10	46 "	1
3 "	5,026	23 "	10	47 "	1
4 "	2,355	24 "	23	48 "	1
5 "	5,112	25 "	102	50 "	18
6 "	1,021	26 "	2	54 "	1
7 "	1,291	27 "	6	55 "	3
8 "	653	28 "	5	60 "	5
9 "	206	29 "	2	61 "	1
10 "	2,316	30 "	73	75 "	3
11 "	77	31 "	1	99 "	82
12 "	337	32 "	1	Life,	1615
13 "	89	33 "	3	Total, 31,925	
14 "	153	34 "	4		
15 "	657	35 "	9		
16 "	65	36 "	2		
17 "	62	38 "	1		
18 "	137	40 "	18		
19 "	26	42 "	1		
20 "	537	43 "	1		

The figures for the even fives and tens in these tables tell their own story. An age that must be determined by estimate, or the length of imprisonment that is best for a particular criminal is a matter that cannot be determined exactly. Approximations by five year periods are as close as many estimators find it convenient to go. In round number estimates of any sort, the number series is not used for its original purpose of enumeration, but merely as a convenient scale by which to indicate quantity, and, as a scale, it is properly used in fine or coarse divisions as the nature of the material dictates.

The notion of what constitutes a round number varies immensely with the nature of the thing to which the number is applied. We give the population of a city roughly as so many thousand; but the attendance at a concert as so many hundred, or at a social gathering perhaps as "eighteen or twenty." Thus, in Dresslar's data, where the guesses mostly fell below 1,000, all guesses in even hundreds are unmistakably round, and to these probably ought to be added those in even fifties, twenty-fives, and possibly also those in even tens. In the present study, where most of the guesses ran above 1,000, it was decided to define a round number guess as one in even thousands, hundreds, or fifties. Of these there were in the

1,043 cases now under consideration, 159, or something over one in seven. These are distributed as follows: In even thousands, 47; in even hundreds, 70 (of which 29 were in even five hundreds); in even fifties, 42.

The proportion of round number guesses falling in the different thousands is shown by the following table, the figures in which are percentages of the total number of guesses falling in each thousand. Guesses lying above 16,000 have been disregarded.

TABLE III.

Percentage of round number guesses falling in the different thousands up to sixteen thousand.¹

Thousands.	Percent. of Round Nos.	Thousands.	Percent. of Round Nos.	Thousands.	Percent. of Round Nos.
0 — 999	3	6,000 — 6,999	11	12,000 — 12,999	17
1,000 — 1,999	21	7,000 — 7,999	13	13,000 — 13,999	0
2,000 — 2,999	19	8,000 — 8,999	13	14,000 — 14,999	20
3,000 — 3,999	15	9,000 — 9,999	13	15,000 — 15,999	27
4,000 — 4,999	12	10,000 — 10,999	18	16,000 — 16,999	20
5,000 — 5,999	13	11,000 — 11,999	21		

It will be noticed that the proportion of round number guesses to the total number of guesses is higher at the ends of the range considered than it is in the middle. Those who guessed numbers between 1,000 and 3,000, or over 10,000, were more apt to guess in round numbers than those who guessed numbers between 4,000 and 10,000. In other words, those going widest from the actual number, *i. e.*, the more careless or less expert estimators, were in general more apt to deal in round numbers. The groups of instances on which the percentages for numbers above 11,000 are calculated are all small, but taking all instances from 11,000 to the upper limit of the group (1,000,000) nearly 22 per cent. of the guesses are in round numbers.

Particularized Numbers. Opposed to this natural tendency to estimate in round numbers is the tendency to particularization induced by the conditions of the "guessing contest." A prize is offered for the guess falling nearest to the actual number of beans in the bottle. The guesser knows that the actual

¹See the foot note appended to Table I.

number must be a definite one, and therefore turns away from the round numbers, which for him are indefinite, toward some particularized number.¹ A round number also seems common and easy to think of, and therefore unlikely to be the right one. Furthermore, the round numbers form but a small part of the whole number series, and the chances are greater that the actual number will be some particularized one than that it will be a round one. A particularized number is therefore chosen, the guesser forgetting—the whole process of naïve guessing is unreflecting—that the chances are no greater that any single particularized number will hit the tale of beans in the bottle than that an adjacent round number will do so.

The operation of both tendencies—that to guess in round numbers and that to guess in particularized numbers—appears in the guessing of numbers lying just above or below round numbers, *e. g.*, such numbers as 7,001 or 10,099. Of these there are fifty instances in the group of guesses now under consideration, some appearing more than once. The distribution is as follows: Numbers ending in -01, 15; in -51, 14; in -49, 6; in -99, 15. The liability of the guessing of any given number (if we leave out of account the scattering guesses above 15,999) is about 1 in 16. The ratio of round numbers guessed to round numbers possible in the same range (285 to 15,999) is 1:3.7; and of numbers lying next to round numbers (with the exception of those ending in -49, which fall below the average for numbers in general) about 1:10 or 1:11. The full tendency to guess numbers lying adjacent to round numbers is not shown by these figures however. The number guessed lies often a little more remote from the round number, *e. g.*, 1,003 or 9,007, but where its character is evident. On the other hand, the tendency to guess numbers ending in -99 does

¹ By a "particularized number" I mean, of course, the opposite of a round number.

The particularizing tendency went so far with some guessers that a half bean was specified in half a dozen cases (*e. g.*, 4,035½), and in two cases a fraction of ¾ was set down. It is very likely that these guesses were made in sport, though possibly they may have been suggested by a stray half bean seen through the side of the bottle. But in any case they are evidence of the strength of the tendency to particularize. All fractions have been disregarded in the statistical study of the guesses.

not rest exclusively upon the fact of their falling just short of a round hundred, as will appear in the on section Repetitional Numbers. Indeed, in view of the small inclination to guess numbers ending in -49, it may be questioned whether the tendency is not almost exclusively toward guessing numbers lying just *above* round numbers.

Repetitional Numbers. Strong as the tendency is to guess round numbers some guessers appear to cut loose from it altogether and determine their choice according to striking features in the visual or the auditory form of the numbers, *e. g.*, the repetition of a single digit, as 3333 or 7777,—a tendency already noticed by Dresslar. Every number of this form between 999 and 11111, appears in our group of 1,043 guesses, and many of them more than once. The ratio of those present to those possible in the range from 285 to 16,000 is 1:1.5.

Many cases also occur where there is partial repetition, the last three or the first three digits of a four place number being alike, *e. g.*, 5222 or 5550. Of the first sort there are 17 different cases out of a possible 72 within the range of guesses now under consideration, a ratio of 1:4.2. Imperfect repetitional numbers with the unlike figure last (*e. g.* 5550) are much less frequent, occurring in only about one-tenth of the possible forms. If a guesser has set down the same digit in the thousands', hundreds' and tens' place, he is much more likely to go on and set it down also in the units' place than to turn off at that point to some other digit.

It is interesting to note that most of the repetitional numbers belong to the upper half of the digit series. This is due in part, doubtless, to the greater frequency with which all numbers belonging to these thousands appear, but it is not to be explained wholly in this way, for the same is true in a measure of the incomplete serial numbers (see the section on serial numbers). As the guesser, in constructing his four place number, ascends the digit series his range of choice becomes smaller and smaller (unless he is to turn back or to begin again) and he is thus pressed more and more to repetition. This would be especially the case when he has reached 9; and of the various repeated digits 9 is of most frequent occurrence, as may be seen in the table of preferred numbers below. With this also

co-operates the tendency to guess numbers adjacent to round numbers, which includes, of course, all numbers ending in 99. Guesses in which the digits are repeated in pairs (*e. g.*, 1212 or 5656) appear in about one tenth of their possible forms.

Symmetrical Numbers. A few instances occur when visual symmetry (or auditory rhythm) may have been a determining factor, as for example, 10101, which occurs twice in this form and once as 10101½.

Serial Numbers. Such tendencies as we have been considering are at least semi-conscious. Guessers yielding to them might, at least, be expected to know what they were about. This is also very likely the case when complete ascending or descending serial numbers are guessed, like 1234 or 9876, but with incomplete serial numbers, as 4789 or 6783 it may be questioned whether this is true.

Considering the range of numbers from the lowest guess in this group (285) up to 16,000, twelve types of complete ascending serial numbers are possible, leaving zero out of account. Five of these are found in the group before us (ratio 1:2.4), several being guessed more than once. Within the same range thirteen types of complete descending serial numbers are possible. Of these four are found (ratio 1:3.3), one of which (6543) was guessed four times.

The incomplete serial numbers have been investigated for the four place numbers only (1,000—9,999). Of these there are four types: 1. Numbers in which the serial part ascends and the non-serial digit comes *first* (*e. g.*, 7123). 2. Numbers in which the serial part ascends and the non-serial digit comes *last* (*e. g.*, 1237). 3. Numbers in which the serial part *descends* and the non-serial digit comes first (*e. g.*, 7321). 4. Numbers in which the serial part *descends* and the non-serial digit comes last (*e. g.*, 3217). Of each of these types there are 57 possible cases in the range considered (zero being disregarded as before). Of the first type 12 different cases are found among the guesses; of the second, 10; of the third, 7; of the fourth, 9 (ratios, 1:4.8, 1:5.7, 1:8.1, 1:6.3). One or two of the numbers of the second and fourth types are guessed more than once.

When it is considered that the general ratio of numbers guessed to all numbers within the limits of 1,000 and 9,999 is

about 1:11, it will be clear that there is a fair tendency to guess numbers of this serial sort. The guessing of incomplete serial numbers is, I believe, the result of the tendency already mentioned to advance in selecting a succession of digits, by short steps up or down the number scale. Even after the removal of all the special sorts of numbers so far considered there remains still some little indication of the serial tendency, though perhaps not more than would be accounted for by serial arrangements of two places (*e. g.*, -67, or -54) which have not been taken into account.

By way of a brief résumé of the influences that we have been considering we may turn to the following table, which includes all numbers guessed three times or more in the group of 1,043.

TABLE IV.

Numbers guessed three times or more.

ROUND NUMBERS.				REPETITIONAL NUMBERS.			
1,500 guessed 7 times.				9999 guessed 7 times.			
2,000	"	5	"	8888	"	4	"
10,000	"	5	"	6666	"	3	"
3,000	"	4	"	7777	"	3	"
6,000	"	4	"	9997	"	3	"
7,500	"	4	"	SERIAL NUMBERS.			
8,500	"	4	"	6543 guessed 4 times.			
9,000	"	4	"	OTHER NUMBERS.			
2,500	"	3	"	7840 guessed 3 times.			
2,850	"	3	"	7989	"	3	"
5,500	"	3	"	10101	"	3	"
7,250	"	3	"				
7,850	"	3	"				
8,000	"	3	"				
11,000	"	3	"				
15,000	"	3	"				

Number Preferences. The round numbers, the repetitional and the serial numbers even with their imperfect types cover, however, about one-third only of the 1,043 guesses of this group. Seven hundred remain which do not fall into any of these classes. For the succession of digits in these we can say no more than "number preference," and can offer but such explanations as can be offered for that.

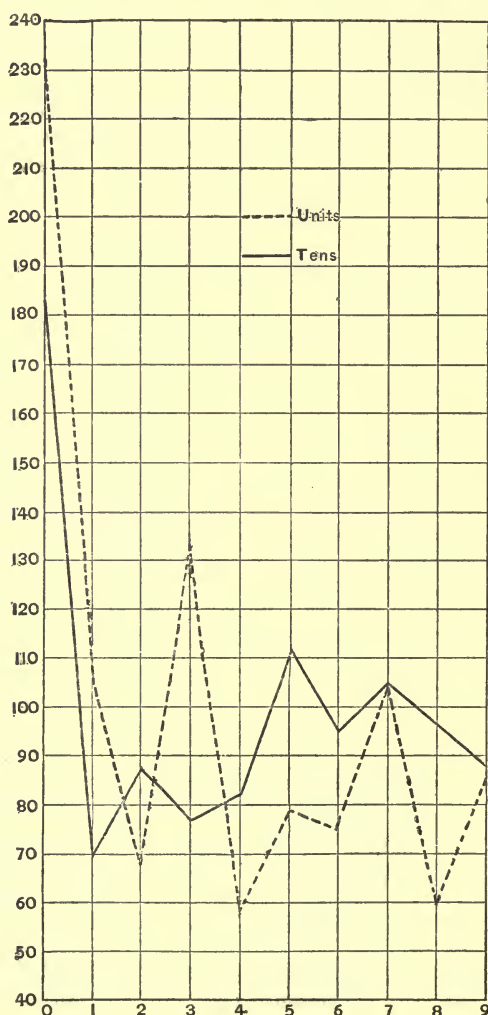
The preferences in the units' place have already been shown in Table II and the accompanying chart. Those in the tens'

place are shown in Table V and the chart below, where for convenience of comparison the frequencies in the units' place are shown again.

TABLE V.

Frequency per thousand of the various digits in the tens' place.

Digits.	0	1	2	3	4	5	6	7	8	9
No. per M.	185	69	88	77	82	112	95	105	97	88



The most striking difference is the partial smoothing of the curve due to the almost complete disappearance of the advantage of the odd numbers over the even. This means, of course, a general lessening of preference. For digits in the hundreds' place the uniformity is still greater. It is evidently the final figure that is most significant in this respect in the mind of the guesser.¹

The order of preference for the digits in the units' place is:

0, 3, 1, 7, 9, 5, 6, 2, 8, 4; in the tens' place: 0, 5, 7, 8, 6, (2, 9), 4, 3, 1.²

The most marked change is the ascent of 5, 8 and 6, and the descent of 3, 1 and 9. Five is favored in the tens' place by the habit of guessing in round 50's; 1 and 9 in the units' place owe their importance, in part at least, to their serving in numbers adjacent to round numbers; and this fails them in the tens' place. For the advance of 6 and 8 in the tens' place and the descent of 3, no reason can now be offered. A digit in the tens' place is, of course, not simply the digit moved one station to the left. It means a different thing and has a different name. The verbal number system is not so simple as the visual system (Arabic figures); 50, for example, is "fifty" not simply "five tens." All of the numbers up to 100 have thus an individuality of their own, and even larger numbers may, perhaps, have something of it.

It was thought that perhaps a preference might show itself for certain combinations of tens and units in the numbers under 100, and a table was prepared giving the frequency for the various combinations. The total number of guesses in the group (1,043), is not sufficient to give precise results when distributed over so long a list of possibilities, but a few points stand out with some clearness. The most general relation discovered was a tendency of high digits in the tens' place to have high digits following them in the units' place, and low digits to have low—a relation which still holds in a slight degree when such serial and repetitional numbers as might contribute

¹ Dresslar's tables show the same relation between the frequencies of the digits in the tens' and units' places, though the advantage of the odd numbers persists to a greater extent than in the figures of Table V.

² Numbers in parentheses have equal frequencies.

to that result are thrown out. The same is true of the succession of digits in the hundreds' and tens' places. This difference is probably no more, however, than would be accounted for by repetitional and serial tendencies confined to two successive digits.

There is a considerable range in the frequency with which the different combinations appear. Leaving out of account the round, repetitional and serial numbers, the following appear as combinations of high frequency, reading from highest to lowest in the first case, and from lowest to highest in the second.

High frequency:¹ 75, (20, 60, 63), 76 (43, 87).

Low frequency: 66, (48, 88, 94), (14, 46, 95).

The number 75 undoubtedly owes its prominence, in part at least, to its marking three fourths of 100; but 25, on the other hand, is not helped in any great degree by marking one fourth.

It was also thought that numbers made prominent as products in the multiplication table might stand high, but no such relation appears. It was thought again that there might be some tendency to serial or repetitional numbers of two places, like 5152 or 4545, since in reading numbers of four places it is not uncommon to make two groups of two digits each rather than a group of one and one of three. Numbers of this type were found, but not often enough to indicate a preference for them. Neither was there any other relation of a general character discovered.

An explanation of number preferences, if one is attempted, must take several things into account. First and most important of these is that number preferences—so far at least as they can be judged by mass returns—are not constant, but vary with the conditions under which the numbers are used. The odd numbers are preferred in the units' place in "guessing contests," but the even (next after the 5's and 10's) in the estimation of ages, and two years is the most frequent criminal sentence. Under some conditions the landmarks of the decimal system (5, 10, 15, 20, etc.) would be prominent; under others those of the duodecimal system; under still others, numbers not belonging to the series of whole numbers at all, as in parts of the country where the "bit" and "shilling" are still

¹ Numbers in parentheses have equal frequencies.

used as money of account. Number preferences should be explained, therefore, in connection with the special circumstances under which they are exhibited.

In explanation of the preference for odd numbers in the units' place in the squash guessing contest, and especially for the prominence of 7 and 3, Dresslar suggests that they may be connected with number superstitions and symbolisms. He remarks, on p. 784 of the paper already mentioned, "it would certainly be unjustifiable to conclude from the evidence at hand that the preferences shown in the guesses under consideration are directly traceable to some such superstition; and yet one can scarcely prevent himself from linking them together." A connection between the two there very probably is, but it lies, I believe, in the fact that both number superstitions and number preferences in free guessing spring from a similar psychical condition. There must be something peculiar about a number to which superstition or symbolic meaning may cling; it must somehow stand out in consciousness. The emphasizing feature may be something in the numerical relations themselves (as 30 is the sum of the first ten numbers of the series, to use one of Dresslar's instances), or it may be some relation in nature, as man's having five fingers on each hand, or the quarter of the lunar month being seven days, or perhaps some purely accidental relation—but whatever its nature, it must make the number prominent in consciousness before it can become a matter of superstitious regard. Now in such guessing as we have been considering, mere prominence in consciousness, or mere ease of return to consciousness, for any cause, is sufficient to determine a preponderant frequency in the guessing. Superstitious importance when once established may easily contribute to the prominence of a number, and so increase its frequency in the records of the guessing, but its influence is indirect and much modified by other considerations. The number 13, for example, would generally be regarded as an unlucky number—though, to be sure, the specific superstition is about sitting thirteen at table—yet in the group of guesses under examination numbers ending in -13 are not avoided at all. On the contrary this termination belongs to the favored group, being guessed more than any other number lying in the

'teens and one-third more than the average. Its unlucky reputation, if it is effective at all, seems in this case to have favored its frequency by making it prominent in consciousness. In such guessing as that now under consideration, the guesser picks out numbers of a certain distinction and passes by those that seem ordinary. All the odd numbers stand out above the even for purely numerical reasons. They present a certain solidity because they are not divisible by two, and among the odd numbers 3 and 7 over-top the rest; for 9 is not prime, 5 is common and easy from its connection with the decimal system, and 1 from its simplicity and complete familiarity. To such original means of emphasis as this is added the repetition and fixation in attention due to superstitious or symbolic conceptions, and all combine to determine the otherwise undetermined digits in the number guessed.

Guesses of the Women and Girls. With regard to the guesses of the women and girls all that need be said is that they did not differ more from those of the men and boys than would an equally numerous group selected at random from the men's and boys' list. On the contrary it is rather remarkable that so small a group should show such slight variations from a large one.

The group consisted of 244 guesses made by 114 persons, an average of a little over two guesses to each person. It differed from the group just considered in containing the guesses of a few persons who guessed more than five times, but the number thus added was hardly worth considering. The guesses range from 250 to 2,675,181,756, the one before the highest being 69,625. The median guess is 7,571+, the median of the upper half series is 8,929+; of the lower 5,827+. The range from 1,200 to 16,000 includes a little more than nine-tenths of the guesses. The percentage of round number guesses in the full 244 guesses is 15.2 per cent. (men's list 15.2 per cent.); of numbers adjacent to round numbers 6.1 per cent. (men's list 5.9 per cent.); of repetitional numbers between 333 and 9,999 4.6 per cent. (men's list 6.4 per cent.); of serial numbers (between 1,000 and 9,999) 5.8 per cent. (men's list 7.7 per cent.).

The following is the list of numbers guessed more than once:

ROUND NUMBERS.	REPETITIONAL NUMBERS.
1,000	7777
5,850	8999
6,500	9999
7,000	
7,450	
8,000	SERIAL NUMBERS.
	6321
MISCELLANEOUS.	6783
9,642	7654

The only number guessed more than twice was 8,000 which was guessed four times.

It is possible, of course, that in a very large mass of data some variation between the guessing habits of men and women might appear, but it is not at all likely. It would be hard to find anything in the mental world more desiccated than the number system, and therefore more unlikely to give ground for differences in emotional attitude, or to discover any form of activity in which the sexes would stand upon so nearly an equal footing of experience as in the guessing of the number of beans in a bottle.

Summary. The data presented seem to me to bring out clearly several points with regard to habits in the guessing of numbers. (1) These habits are not fixed and constant, as seems generally to have been assumed, but vary characteristically with variations in the conditions under which the guessing is carried out. (2) Two-thirds of the guessers in the "guessing contest" here studied made use of particularized numbers, showing more or less preference for certain digits, especially in the unit's place. (3) About one-third guessed round numbers or those adjacent to them, or numbers showing a repetitional or serial character in the digits chosen. There was also a slight, but uncertain, indication of a more general tendency to move, in choosing a series of digits, by short steps along the digit scale. (4) No evidence was discovered that the guessing habits of women and girls differed from those of men and boys.

A QUARTER CENTURY OF PSYCHOLOGY IN AMERICA : 1878-1903.

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It was almost a century before the beginning of the period we wish to review that Kant, supported by all the machinery of his critical analysis, declared psychology to be forever an impossible science. The facts of the mind being given in time alone, and in no wise under the conditions of space, and *therefore* existing without the pale of exact measurement, was the reason upon which he based his assertion and his prophecy. This ideal of a scientific psychology was far more worthy than the prophecy to which it led. The century that has just passed possesses no greater merit in the changes of human thought and achievement than that which rendered this prophecy inert, while cherishing and realizing this ideal. American scholarship and energy have in part reflected, and in part contributed to, these great changes, to review which is fitting on this anniversary occasion.

There were two lines of constructive influence which operated directly in shaping the trend of psychology in America during the first three quarters of the nineteenth century. These may be summarized in terms of the speculative and the empirical aspects of thought which prevailed in the eighteenth and the early part of the nineteenth centuries. The former mould of belief and endeavor was derived from Germany; the latter, from England. The former was the descendant chiefly of the reasonings of Kant, Fichte, Hegel, and Herbart. The latter represented the best efforts of Locke, Hume, Reid, James Mill and his son. The one mode of psychologizing was to apply the fundamental tenets of "reason" to consciousness, principally as found in man. A priori conceptions were given application through persistent deductions. The other mode reveled in the endless sport of an individualistic analysis of human consciousness. Both of these developments in psychology were

extremely individualistic, than which no quality can be more baneful to any attempted science. Still another line of psychological development was characteristic of French science. Here the human mind was approached through medicine and special interest in certain types of abnormal consciousness. Mental pathology was variously exploited in extreme ways. These three national tendencies have severally and collectively been influential in awakening and guiding American efforts at different times and into different directions in the total field of consciousness. The chief item in this historical glance is to be noted in that unique speculative preparation for a scientific psychology which was made by Herbart's elaborate and involved efforts at a "mechanic" and "static" of mind.

Prior to the beginning of our special period, psychology in America, borrowing heavily from British and German sources, predominated in the theological turn given to its speculative type. Universal hypotheses concerning the soul were turned to account chiefly by the theologians. Religious interests profoundly stirred our colonial forefathers in both a philosophical and an academic sense. Edwards, for example, was the chief theological psychologist we had produced. Closely identified with this union of psychology with theology, there also appeared the educational aspects of the "philosophy" of mind. Education and psychology became closely related very early in American development, but in a manner radically different from that which, as we shall see later, is actively contemplated and practiced in the present day. Psychology was taught in the higher schools then existing,—not by psychologists,—but by pastors. The Puritan traditions in favor of religious education absorbed soul lore with a peculiar appetite. Men spoke unblushingly of "the soul." And they meant by it the same thing in their theology as in their philosophy. The object of study flourished under the somewhat dubious terms of "mental philosophy" and "moral philosophy." Psychological knowledge, as this has since become distinctly recognized, occupied but a small, unnamed portion of the entire field of philosophy. The abiding result of this dogmatic naïveté was an enlarging credit to, and confidence in, consciousness—chiefly human—which has practically saved it from

shattering under the repeated attacks of a later scientific scepticism. Men thought of the mind as of a thing. They gave it a primal reality. The language they applied to it was profoundly more than a mere symbolism. "Fundamental beliefs" and "intuitions" could be drawn from its depths with the grappling hooks of speculative analysis as readily and as inexhaustibly to suit the needs of any peculiar occasion, as water could be drawn with the old well-sweeps. One can say all this without being pledged to support the common sense realism of Scottish thinkers, which was the philosophical background of most, if not all, the adapted psychology which became current in American educational and theological circles.

Some typical books, which may be mentioned as evidence in this connection, were Tappan's "Doctrine of the Will determined by an Appeal to Consciousness" (1840), Hickok's "Rational Psychology" (1848) and "Empirical Psychology" (1854), and Porter's "Human Intellect" (1868). (The last volume contained some progressive departures from the early prevailing type, having been written in acquaintance with the then growing knowledge of physiological facts.) Another striking feature of this early type of American psychology is the fact that its books were written by men who were primarily theologians and educators, authors whose profession combined in one person the functions of the chair of mental and moral philosophy and of the president of the college, and of a preacher. In these and many other books "*the mind*" was treated in terms of "faculties," distinct and separable "powers." Consciousness was "ready-made," a sort of "hand-me-down" affair, which could be had for the studious and persistent asking. The power and authority of authorship were far more current and important in those days than the power and authority of free, independent investigation of mental facts and their correlated conditions. Fichte had quite a modern inspiration when he critically declared that men were prone to believe some old book rather than their own consciousness!

How can psychology become a science? No question of the intellect has been approached with more persistent gropings since the time when Descartes and Locke attempted to answer it. At first men seemed to think that the content of the science

had to be made in order to be able to respond to the query. It was not until the beginning of the third quarter of the last century that the question radically appeared to be one of method rather than of conception. The world-old veneration for Reality, Being, Becoming, the Absolute, slowly gave way before a growing regard for the *facts in* consciousness and for the *facts about* consciousness. The revolution inherent in this iconoclastic change resulted in, and is well chronicled by, the labor which produced such works as Lotze's "Medicinische Psychologie" (1852), Fechner's "Elemente der Psychophysik" (1860), Helmholtz's monumental inquiries into vision and audition, and Wundt's "Grundzüge der Physiologische Psychologie" (1874). A wider philosophical platform for the development of the newly explorable psychological interests was provided in the beginnings of the "Revue Philosophique" by Th. Ribot (1876), and "Mind" by G. Croom Robertson (1876). The active and established possession of the new field was effected by the Psychological Institute at Leipzig, founded by Wundt in 1879. This brings us within the beginnings of our quarter-century, and the "new" psychology is well secured, awaiting its extension over the whole domain of human activity as relating to psychological instruction and research. The most striking feature in all this is, that psychology, distressed by the buffetings from philosophical systems and the scourgings of those intellectual vandals who had ruthlessly employed it to destroy the very experience which it, above all other disciplines, supremely validates, secures both peace of mind and a non-forfeitable lease upon life by appealing to the physical and physiological sciences, and adapting the suggestions in their methods and conclusions to its own special problems.

Facts, however, never stand alone before the human intellect. Almost simultaneously with this revolution in scientific attitudes towards the content of daily psychical experience, there swept another revolution over human interests, which,—likewise affecting psychology, but more slowly,—left its reconstructing effects upon all science, as, indeed, upon all practical activity. Facts soon become related, or attached, to hypotheses; and this second revolution consisted in a construction of a universal hypothesis for all science. Spencer's "Principles of

Psychology" appeared in 1855, applying an interpretation of evolution to consciousness. No greater intellectual infusion into psychology has saturated the labors and constructions of the last twenty-five years. Spencer's views as to mind may not have been acceptable to his contemporaries of the 70's and 80's, for it is probably true that Darwin has exercised the greater influence upon scientific views and activity; but the demand that the data of experience should be viewed as the functional changes of consciousness was yielded to. Mind was thus brought anew into intimate relations to universal processes. Psychology acquired a consummating unity with the other sciences. Consciousness became "genetic," and "faculties" were appreciable only as growths. The further demand for unity in naturalistic evolution, so thoroughly expressed in Fiske's "Outlines of Cosmic Philosophy" (1874), was well in the ascendancy, under the leadership of which psychology awakened to a deeper and wider interest in consciousness of every order and of every type wherever manifesting itself. Under the direction of the newly acquired methods of experimentation and demonstration, and the inspiration of the fresh conception synthesizing all the facts, consciousness came to be viewed as a unitary process, better known as psycho-physical. Indeed, mental process, slowly but surely, came to mean the activity of adaptation springing from within, instead of a mere reaction upon an environment. Consciousness, having ceased to be regarded as an entity, possessing parts and properties, is now known as a life, identifying itself with life relations.

When we come to look for the features of the psychology which has come to be among us, we find them to have developed through a devotion to measurement, enumeration, and comparison, as the efficient methods of ascertaining the elemental facts of the inner life and their relations. The experimenter, who teased out the intricate adjustments between impression and expression,—the two primary functions in reactions,—first claimed the field as his own by reason of the facts obscured in daily living which he brought to light. But the need of the method of the statistician, who can determine tendencies by taking faithful recognition of frequency of occurrence among mental processes, gradually appeared as the

experimenter reached his limits with the so called lower processes of mental action. To find out truly what the mental life, in its multiple varieties *is*, has been the constant aim of those who proceed by precise laboratory methods or extensive questionnaires. The results of both types of students must be carried forward in credit to the scientific interpretation of that experience from which they were originally derived. Exactness in statement of facts has probably been the idolizing passion among the progressive *Fachmänner* trained in the earlier years of our period. Objective conformity in discoverable facts is the test for off-setting the peculiar limitations of merely individual observations, however honestly acquired, and for eliminating the warping bias of prejudice, to which the study of mental phenomena has ever been prone. No one can fail to be struck by the fact, that, with all the machinery of laboratory precision and with all the ingenuity of asking questions on unsuspected items in one's experience, every psychologist has been meeting the prime condition of the science, namely that it shall be true descriptively. To get a true picture of the mental life, describable in terms of human speech, has unquestionably been the persistent aim throughout these years, in which American investigators have cordially joined with their European colleagues.

The two types of methods just referred to have received a large addition in more recent years through the necessity which has led to the introduction of what may, by contrast, be called the comparative method. This method has enlarged the domain of psychology by leading its devotees to search the conscious manifestations in the lower forms of life, to undertake a more critical and exhaustive observation of the mental behavior in the higher animals, and finally to see the life of civilized man constructing itself through the modes of reaction, impulse, and deliberation which have become traceable in primitive men and children of civilized races. Almost the entire departments of child, race, and animal psychology, which now are so splendidly equipped with a knowledge of their respective objects, have been either entirely reconstructed or newly created within these twenty-five years, as a direct consequence of an earnest appreciation of the potency of

comparison in the hands of a trained psychologist. The thorough study of sensory-motor processes and their multiple relations is to be credited to the first type of methods, which also has thrown brilliant light upon the higher manifestations of mind, completely overshadowing the painfully exhaustive emphasis placed upon the intellectual processes by the genius of the earlier psychology. The method of comparison has also brought a new light upon the obscure but wide-reaching life of impulse, instinct and volition, opening before us a larger world of mind than the most hopeful of the earlier psychologists ever dared dream of.

Among all people scientifically alive, during the quarter century, psychology has also been taking rapid strides beyond the primary task of description. All this application of these methods has steadily progressed under the power of that logical belief which has pervaded the thinking of the explorers in the fields of physical science. The same logical principle of inductive generalization, which, according to Mill, controlled in the fabric of the material sciences, has gradually assumed its leading rôle in psychological science. The principle of "the uniformity of nature" became applicable to mind through these many years of effort at exact and exhaustive description of mental processes, a few of which had been so often analyzed and re-analyzed in earlier eras in psychology. Explanation of the facts which are faithfully described is the larger and more intricate task of every science; and psychology has steadily faced this duty during the last twenty-five years.

In the first place, the careful description and attempted explanation of specific mental phenomena have proceeded with astonishing progress. Psychologists were altogether too late in the progress of science in discovering that their first duty lay rather in the direction of picking out detachable facts and studying them, than in that of attempting to discover some mode of universal explanation for the science. In numerous particulars Aristotle established good models in this respect, which were all too soon neglected and then totally forgotten. To select distinct phenomena, which may be studied apart and explained as integral problems, such, for example, as the perception of letters and angles, psycho-physical reflexes, special

instincts, the horopter, control of the *Eigenlicht*, attention and contrast, simple voluntary movements, special associations and memories, the displacement of cross lines in visual illusions, imitation, tone discrimination, etc., etc., is to travel securely along the avenue of progress in exploiting the new region opened by the new methods of research and demonstration. The chief benefit arising from such zeal is to be found in a sharper definition of the field for psychology, increased accuracy in and enrichment of the actual knowledge the science possesses.

In addition to this alertness for the selection of specific phenomena which may be detached and studied, there has appeared an increasing recognition of that larger duty of explanation in psychology which should find for the whole science some principle which stands in an explanatory relation to the extreme compass of its investigation. This tendency towards the perfection of a supreme generalization has moved in two directions. The largest issues of this epoch appear in the varying answers individual psychologists and the nascent "schools" of psychology have given to these two questions: (1) What explanation, if any, shall be given of the relation of the activities of the individual consciousness to the particular brain processes with which they are always connected in experience? (2) What is the relation of the structure and function of mind at present to minds in the past? Do minds stand in a constructing series, each unit of which absorbs the fundamental activities of all the preceding units? The first is the problem of brain psychology; the second, that of genetic psychology. The adoption of one theory resolves all orders of consciousness into a psycho-physical process; of the other, into a bio-genetic process. The one is satisfied in averring unity of the total individual organism. The other maintains a belief in the unity of the race of organisms. The one applies logical principles of interpretation to specific processes. The other gives a cosmic evaluation to the psychic half of every form of life. The former may be said to have been a dominating theoretical tendency during the first decade and a half of our period. The latter appeared with vigor a decade ago, and is now unquestionably in the ascendancy.

Another essential feature in the explanatory tendencies in the psychology of these later days may be illustrated by the contrast and the debate between the Associationists and the Apperceptionists, who still maintain their camps on a war footing. To the Associationist mental forces mean something after the analogy of physical force in the objective world. The Apperceptionist holds that mental processes are expressions of a distinct cosmic function, which has a validity equal, at least, in the objective world to the play of forces in atoms, molecules and masses. These debates in search of a complete psychological theory, which shall offer a satisfactory explanation of the interaction of mental processes and their universal conditions, reflect a wholesome state of the science so long as they are conducted primarily in the interest of psychology and always on a basis of fact.

Next to the fact that a definite and positive science of man, growing in facts and in theories, to which every special investigator may offer somewhat permanent contributions, has been created, the most interesting feature of the last twenty-five years, both in Europe and America, is the changed relation between psychology and philosophy. This readjustment found its chief exponent, perhaps, in the large and persistent conception and labor out of which grew this *Journal*, from the first devoted and pledged to the elevation of psychology through a sympathetic reciprocity of all lines of the objective study of mental facts. In the past psychology was given a small corner in a philosophical system. The approach to a knowledge of mind lay through the fields of abstract conceptions, of which those presumed to fit such an entity had to be analyzed and adjusted to other abstractions before a discussion of subjective experience could be entered upon. *Then* one's psychology grew out of, and was dependent upon, his philosophy. *Now* one's philosophy depends upon his psychology, upon his recognition of the psychological facts of experience and his methods of interpreting them. Indeed, a man may nowadays not infrequently be found who possesses a psychology but no philosophy as such, the latter having been completely and enthusiastically absorbed by the former. What is true for all men now is, that no one who has not blown the psychological trum-

pet can gain the attention and respect of his philosophical contemporaries.

This complete change in the pointings of our intellectual compass does not do violence, so often feared, to either psychology or philosophy. Great value to both has accrued through a separation of mental and speculative interests for the sake of exhaustive knowledge. Ontological and other philosophical interests are not done away with. The new psychology only assigns them functions, different from those prevailing in earlier epochs but more progressive, in the new administration of the kingdom of truth. While philosophy has slowly, but certainly lost psychology forever, it has gained the opportunity of ceasing to be a pet affair of individual abstractions and of assuming a content full of life. The benefit also extends to the teaching of psychology and philosophy, each of which has made much progress. Vivacity and concreteness now replace—or can replace—the old weary grind of dogmatic jingle.

We come now to a recital of some of the most interesting and characteristic events in the history of psychology in America during the last quarter century. These items relate to university interest in the science, the founding of laboratories, the developments in organized instruction in undergraduate and especially in graduate courses, the widening influence of these changes upon the practical issues of human life, particularly education, the establishment of journals and the production of a technical literature, consisting of articles, monographs, treatises, and text books, and the promotion of the welfare of the science through the corporate activity of the American Psychological Association.

Probably the earliest attempt at exact demonstration in psychology in America occurred at Harvard University. Professor James has said that it was either in 1875 or 1876. But the first laboratory for psychological demonstration and research was opened in 1883 by him in whose honor this *Festschrift* is inscribed, during his incumbency as professor of psychology and pedagogy in the Johns Hopkins University. Four years later *The American Journal of Psychology* was founded by Dr. Hall himself, with the aim already mentioned, and "The Elements of Physiological Psychology" was published by Professor

George Trumbull Ladd, of Yale University. This *Journal* and this treatise, by reason of their scientific vigor and the sanity of the industry they represented, completely effected a wholesome American adoption of all that was good of the progressive movements in foreign lands at that time, and also provisioned the integrity of the high purposes of American psychologists to work independently of foreign systems and thinkers. In 1888 Professor J. McKeen Cattell occupied the first chair in America created for psychology alone, having a laboratory attached, at the University of Pennsylvania. Here the first American college students received instruction in experimental psychology. Thus at the end of the first ten years of our quarter-century we meet with those conditions which peculiarly and effectively adapt a new and growing science to American ways and needs; namely, a journal, a book, a workshop, a teacher, and a college class! The pedagogical adaptation of the science, which is an American peculiarity, advanced still further in a very decisive manner when Professor E. C. Sanford began the publication of his valuable "Laboratory Course in Physiological Psychology" (1891). This publication was intimately connected with the activity in psychology at Clark University, which had been opened in 1889, and to the presidency of which Dr. Hall had been called in 1888.

Many public events tending to promote the welfare of the science and the extension of its influence upon modern life became closely connected with Dr. Hall's labor and initiative. His services as a teacher of university students and a guide and help to many a youthful investigator have likewise continued with unabated vigor. In 1891, he founded The Pedagogical Seminary, which has been, and is, devoted to the growth of educational thought as an application of a sound psychology, and has been a chief repository of many studies in child psychology. The same year he conducted a round table on psychology at the Toronto meeting of the National Educational Association of the United States. Again, in 1893, he organized the section on experimental psychology and education as a part of the Congresses held in connection with the Columbian Exposition. In 1892, Dr. Hall, hoping to promote still further the interests of psychology in America by uniting its

workers into closer bonds, planned, in conference with Professor Ladd and others, a society of psychologists, and invited more than a score to meet at Clark University on July 8th, 1892. As a result of this meeting there was organized The American Psychological Association, which has gradually become the largest and the most important single factor incorporating psychology into the temper of American institutions, both scientific and educational.

This Association has held meetings annually since its organization, having been the guest of the leading universities in the country until its recent affiliation with the assemblage at Convocation Week. It has had a membership of one hundred and forty-eight psychologists, eighty-nine of whom have been contributing members. In the course of its history the Association has received two hundred and eighty-three communications. How completely the scope of these papers accords with the extending bounds of the science is to be seen from the following summary which shows their percental distribution over the chief topics :

General Topics,	20%	Characters of Consciousness,	4%
Sensation,	19%	Mental Tests,	4%
Genetic, Social and Individual,	14%	Sleep, Trance, and Pathology,	3%
Higher manifestations of mind,	14%	Anatomy and Physiology of	
Cognition,	12%	the Nervous System,	3%
Conation and movement,	6%	Affection,	1%

Twenty-five years ago America did not possess a single laboratory for psychology, although there were instruments of precision and graphic means for demonstration among us. In nine years after the establishment of the first laboratory on this side of the Atlantic Ocean, there had been fifteen laboratories equipped either for research or for demonstration. In the next two years ten new laboratories were opened. Now there are experimental facilities found in not less than forty colleges, universities, and pathological institutions in the United States. Still other colleges and normal schools have varying grades of facilities for teaching the science. And since the statistician has come in with his questionnaire, there is hardly a school but has its worker gathering or collating some new facts, or verifying some older studies. One of the most inter-

esting features in this rapid and almost luxurious extension of the facilities for psychological investigation, which had centered chiefly in our great universities out of purely scientific interest, is that the adaptation of the exact methods of collecting facts in our universities, normal schools, and educational departments, and the institutions for the insane and defectives, was made almost simultaneously. There has also somewhat recently appeared a tendency to admit instruction in psychology in our theological seminaries as a part of the future minister's training ; but the adaptation of the science to the practical aspects of the religious life remains a development of the future, which may come soon in consequence of the very recent active explorations in the psychology of religion.

If one desires evidence to believe that the extensive equipment for psychology in America—probably the most expensive of any nation—and the special training of psychologists both within and without the laboratory, have been yielding creditable returns, he need but look to the periodicals which have been established especially for receiving the more technical studies conducted in these laboratories, to those which welcome a more popular discussion of psychological facts and problems, to the technical journals of other branches of science which open their pages to the treatment of topics which have mutual bearings with psychology, and to the serial issues and bulletins of universities and laboratories which secure a more immediate publication of their own studies respectively than could occur if this depended solely upon the larger journals. Mention can be made of this *Journal* (1887), "The Psychological Review" (1894), and its nineteen monographs, the publication of which began in 1895. Yale and Iowa Universities each maintain separate "Studies" for their departments of psychology. Chicago, Columbia, Colorado, and Cornell, have special serial issues devoted to philosophy, psychology, and education, or to psychology and education. "The Open Court" (1887), "The Monist" (1890), and "The Philosophical Review" have been valuable adjuncts to the broadening and deepening of psychology and its influence upon cognate departments of thought by having fostered the modern spirit of the science and brought out many valuable discussions of

its topics. The educational press, headed by "The Pedagogical Seminary" and "The Educational Review" (1891), has also performed extensive service for a sound psychology in the schools. Not the least interesting and instructive part of this sketch, could its details be exhaustively presented, would be an enumeration of the many times foreign scientific periodicals have extended cordial welcome to pieces of work done by our students at home.

The literary activity of our psychologists has extended far beyond the bare limit of a supporting contribution to these home periodicals. Systematic treatises, monographic essays, and text books for the use of classes and private students have followed in an almost steady stream from the pens of our psychological scholars and experts. McCosh's "Psychology: The Cognitive Powers," Bowne's "Introduction to Psychological Theory," and Dewey's "Psychology," appearing in 1886 and 1887, were among the first works which effected something of a transition from the old state to the new scientific aspirations, by introducing a recognition of objectively valid facts into the older systematic analyses of the mind. James's "Principles of Psychology" (2 vols., 1890), so long delayed in completion, possessed the double merit of working towards and looking forward to the constructive future of the science, and of placing the charm and persuasion of an attractive exposition of the science in the lead of the world. Tracy's "The Psychology of Childhood" (1893), Ladd's "Psychology, Descriptive and Explanatory" (1894), Baldwin's "Mental Development in the Child and the Race" (1895), Scripture's "The New Psychology" (1897), Harris's "Psychological Foundations of Education" (1898), Titchener's "Experimental Psychology: A manual of Laboratory Practice" (2 vols. 1901), and Baldwin's "Dictionary of Philosophy and Psychology" (1901-1902), may be named as representative of successive forward movements in systematizing the gradual increments which experimental, comparative, and historical research has yielded in these recent years. Other works, less systematic, but too numerous to be mentioned, were produced especially in the 90's.

During the first seven years of the last decade in our quarter-century not less than eight excellent text-books for the science

were prepared by our leading experts. Several "primers," even, by our authorities found their way into the hands of the boys and girls in our secondary schools. And women's clubs and Chautauquan circles have no less been exploited by the ingenuity of American authorship in psychology. No literary opportunity, it would seem, has been allowed to pass unimproved for making all men know something of themselves.

The American reader and student has also had a splendid foreign literature placed at his disposal during the same period largely through the industry of American translators. Eight works by Ribot, two by Preyer, two by Wundt, two by Groos, one each by Külpe and Ziehen, have become an integral part of our literary assets in abnormal, genetic, physiological, and systematic psychology. Our obligations to the psychologists of England during these years are too extensive to be specified in the space at our command. The early debt to French and German psychologists which America incurred in the early part of the last century, and which was so greatly increased in the 70's and 80's began to be repaid in the 90's. Since 1896, no less than seven important American works have been translated into either French or German; and foreign publishers continue to seek for further privileges from our authors. From this participation in an international exchange, may we not look forward with confidence to a time when psychology shall move forward with an unprecedented progress, and to a time when a complete science of man shall be at the command of all men?

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SUBJECT INDEX.

- Æsthetics, 118; experimental, 479. See also Music.
American Psychologists, Statistics of, 579.
L'Année psychologique, 118.
Anthropological. Anthropological Instruction in Iowa, 264; Institutions of the Ogowe tribe, 262; Primitive taste words, 410; "The Human Race," 261; Tsimshian texts, 119; Uganda protectorate, 119.
Apparatus: Air-compressor, 107; Ergograph, 509. See also Experimental.
Archiv für die gesammte Psychologie, 264.
Attention waves and fatigue, 541.
Audition: Absolute pitch memory, 553. See also Music.
Bibliography of questionnaires, 97; of writings of President Hall, 647.
Binocular vision and the problem of knowledge, 306.
Biological, 261; Life, 1, 258. See also Evolution.
Characters and their classification, 261.
Child psychology, 21, 88, 96, 257, 264; Acquisition of the winking reflex, 237; Child Study at Clark University, 96.
Children, neurasthenia and hysteria in, 264; Reactions of, to light and darkness, 21.
Chords and melodies, Memory of, 568.
Chromæsthesia, 632.
Color tones, 92.
Contemporary psychology, 260.
Correspondence, 252.
Darwin, More letters of, 259.
Dreams, 271.
Emerson, Influence of, 264.
Ethics: Höffding's Ethics, 263; Ethical element in the Æsthetics of Fichte and Schelling, 264; The cardinal virtues, 263.
Evolution, Mental, 261; Heredity, 118, control of, 261; Variation in Animals and Plants, 261.
Experimental Psychology, Cultural aspects of, 256; Class Experiments and demonstrations, 439. See also Apparatus and Physiological.
Fatigue tests, 496; Fatigue and attention waves, 541.
Feeling, 13.
Fusion, 324.
Guessing of numbers, 681.
Habit, 121.
Harvard Studies, 118.
Heredity, see Evolution.
Historical: A quarter century of American Psychology.
Hume, 263.
Hypnotism, 116; suggestion, 116, 117.
Imagery, 117.
Imagination, 260.
Imitation, 261.
Inhibition of wink, 230.

- Intelligence and motor power, 615.
- Language, psychology of, 257.
- Learning of muscular dexterity, 201.
- Legal medicine; case of Czolgosz, 117.
- Life, 1.
- Logic, 118.
- Lying, 257, 262.
- Medical: Causes of death, 261; The health of celebrated literary men, 117. See also Legal Medicine and Pathological.
- Memory: Of chords and melodies, 568; Retroactive amnesia, 382.
- Mental arrangement, 113.
- Mental evolution, 261.
- Mental images, 262.
- Mimetic movements, 260.
- Mind and body, 263.
- Modern spiritualism, 116.
- Moon fancies, 88.
- Movement: Motor power and intelligence, 615; genetic function of movement, 337. See also Mimetic movements.
- Music: Aesthetic effects of final tones, 456; Intonation of musical intervals, 461; Memory for absolute pitch, 553; Pitch discrimination, 553; Quarter tone music, 471; Unmusicalness, 561.
- Nervous diseases: Neurasthenia and hysteria in children, 264; Neurotic constitution, 354. See also Pathological.
- Neurology: Nerve cells and cells in general, 264.
- Number guessing, 681.
- Organic sensations, genetic function of, 337.
- Pathological: Dementia præcox, 117; Epilepsy, hysteria and idiocy, 258, 262; Obsessions and impulsions, 260; Obsessions and psychasthenia, 262.
- Philosophical, 115; Agnosticism, 254; Aristotle, 259, 263; Chinese philosophy, 115; Epistemological, problem of knowledge and binocular vision, 306; Methodology and epistemology, 264; History of philosophy, 260; Nietzsche and his doctrines, 119; Problems of philosophy, 263; Reality and deception, 288; Spinoza's political and ethical philosophy, 262; Systematic philosophy (Marvin), 264; The God of Plato, 262.
- Physiological, 1; Blood corpuscles, 1; Plethysmography, 13; Winking reflex, 230.
- Play, college foot ball, 368.
- Probabilities, Laplace on, 258.
- Questionnaires, bibliography of, 97.
- Royce's Outlines of Psychology, 260.
- Religion: Christian mystics, 397; Immortality, 116; Religion and Theology, 115; "State of Death," 397; Theology of Hume, 263.
- Short hand, 224.
- Sleep, 117.
- Social psychology, 118; Sociology, 259; Social consciousness, 337; Subconscious, 343; Summaries and indexes, 84, 252.
- Taste, sense of, 263.
- Time judgments, 418.
- University of Colorado Studies, 264.
- Vision: Accommodation and Conveiances, 150; Depth perception, 150.
- Vivisection, 258.

NAMES OF AUTHORS.

(The names of those who have contributed original matter are printed in SMALL CAPITALS.)

- | | |
|--|---|
| <p>Aikins, Herbert Austin, 118.
 Allin, Arthur, 264.
 ANDREWS, B. R., 121.</p> <p>BAIRD, J. W., 150.
 Battin, Benjamin F., 264.
 BEAUNIS, H., 271.
 BENTLEY, I. MADISON, 92, 113, 324.
 BERGSTRÖM, JOHN A., 510.
 Bigelow, John, 117.
 Binet, Alfred, 118.
 Boas, Franz, 119.
 Bolton, T. L., 615.
 Bose, J. C., 258.
 Bourneville, — 258, 262.
 Bovet, Pierre, 262.
 BUCHNER, E. F. 666.
 BURNHAM, WM. H., 382.</p> <p>CATTELL, J. MCKEEN, 574.
 CHAMBERLAIN, A. F., 410.
 Channing Walter, 117.
 Clevenger, S. V., 261.
 Cooley, Charles H., 119.
 Cuyler, E., 260.</p> <p>Darwin, F., 259.
 Dresslar, F. B., 632.
 Dreyer, Frederick, 264.
 Duff, Robert A., 262.
 Dugas, L., 260.
 Duprat, G. L., 257, 262.</p> <p>EDGEELL, BEATRICE, 418.
 ELLIS, A. C., 496.
 Emory, F. L., 258.</p> <p>De Fleury, Maurice, 117.
 Flint, Robert, 254.
 Fouillée, Alfred, 119.</p> <p>Gaillard, Gaston, 263.
 Garner, R. L., 262.
 GAULE, JUSTUS, 1.
 Gould, George M., 117.
 Grasset, — 116.</p> | <p>HALL, G. STANLEY, 21, 88, 96.
 Heysinger, I. W., 115.
 Höfdding, Harold, 263.
 HVSLOP, JAMES H., 306.</p> <p>Jahrmärker, Max, 117.
 Janet, Paul, 115.
 Janet, Pierre, 262.
 JASTROW, JOSEPH, 253, 343.
 Johnston, Sir Harry, 119.</p> <p>King, W. A., 261.
 KIRSCHMANN, AUG., 288.
 Kronecker, Hugo, 119.
 Kronthal, Paul, 264.
 KUELPE, O., 479.</p> <p>Laplace, Marquis de, 258.
 LEUBA, JAMES H., 397.</p> <p>Mallock, W. H., 115.
 Marchaud, L., 263.
 Marvin, Walter T., 115, 264.
 Mead, Edwin D., 264.
 MEYER, ADOLF, 354.
 — MAX, 456.
 Moore, A. W., 119.
 Morgan, Charles H., 115.
 Matora, Yujiro, 593.
 Münsterberg, Hugo, 118.
 Myers, W. H., 116.</p> <p>Nissl, Franz, 119.</p> <p>Orr, James, 263.</p> <p>Paget, S., 258.
 PATRICK, G. T. W., 368.
 Patten, Simon N., 118.
 Pearce, Haywood J., 119.
 Philippe, J., 262.
 Piat, Clodius, 259, 263.
 PILLSBURY, W. B., 541.
 Pitres, A., 260.
 Podmore, Frank, 116.
 Prat, Louis, 118.</p> |
|--|---|

Ramalcy, Francis, 264.

Raymond, F., 262.

Redfield, Casper, 261.

Régis, E., 260.

Ribéry, Chr., 261.

Royce, Josiah, 260.

Saenger, Alfred, 264.

SANFORD, E. C., 86, 647.

Séailles, Gabriel, 115.

Seward, A. C., 259.

SHIPE, MAUD M., 496.

Sidgwick, Alfred, 118.

SMITH, THEODATE L., 21.

Stalker, James, 263.

Steel, Richard, 261.

STEVENS, H. C., 13, 256.

Stratton, George M., 256.

Strong, Charles A., 263.

SWIFT, EDGAR J., 201.

Taylor, S. Earl, 115.

— Thomas E., 115.

TITCHENER, E. B., 84, 253, 439.

Truscott, F. W., 258.

Turner, William, 260.

Vernon, H. M., 261.

Villa, Guido, 260.

Ward, Duren J. H., 261, 264.

— Lester F., 118, 259.

WASHBURN, MARGARET F., 337.

WHIPPLE, GUY M., 107, 553.

WILSON, LOUIS N., 681.

WINTER, P. E., 254.

Wundt, Wilhelm, 119, 257.





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